Hydrol. Earth Syst. Sci. Discuss., 6, C2413–C2422, 2009
www.hydrol-earth-syst-sci-discuss.net/6/C2413/2009/

Hydrology and Earth System Sciences Discussions

HESSD

6, C2413–C2422, 2009

Interactive Comment

Interactive comment on "Water availability, water demand, and reliability of in situ water harvesting in smallholder rain-fed agriculture in the Thukela River Basin, South Africa" *by* J. C. M. Andersson et al.

J. C. M. Andersson et al.

jafet.andersson@eawag.ch

Received and published: 19 October 2009

General interactive comments

We thank referee #3 for helpful comments on how to improve our manuscript.

Comments 1 to 7: No questions or detailed suggestions hence no further response.

Comment 8: "Does the title clearly reflect the contents of the paper? Suggest changing: Assessing uncertainty in use of insitu water harvesting by smallholder farmers in Thukela, SA by using SWAT modeling tool"

Response: We prefer the original title because it more clearly reflects the objective and major results of the paper.

Comment 9 to 12: No questions or detailed suggestions hence no further response.

Comment 13: "13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See attached draft. Move section page 4934 line 25 to page 4935 line 9, and fig. 7, to a Supplement section: This section, although relevant, cannot be directly compared as the two models use different landuse for input. . . In addition, the authors do not attempt to explain in detail why the models diverge when exceeding 250mm runoff (fig ."

Response: We removed the section as suggested (see also Comment 1 P.4935 of the article supplement)

Comment 14: "14. Are the number and quality of references appropriate? Yes. Some references could be added to Table 1 (if the intention is to be comprehensive)"

Response: We added Mbilinyi et al. (2007) and Mati et al. (2006) as suggested.

Comment 15: No questions or detailed suggestions hence no further response.

Comment 16: "The land use: a fairly small proportion of the basin is crop systems (8+8+2 %), and even a smaller % is the focal area, smallholder rainfed farming system: It would be good to have some idea of the overall model sensitivity to principal changes in these focal areas: If CRC is changed, how does this affect overall runoff? How does this affect the uncertainties? This is where the principal change of in situ WH would manifest itself in the model"

Response: We have added a table showing the results of the sensitivity analysis and further explained the sensitivity in the methodology section. The sensitivity refers to

HESSD

6, C2413–C2422, 2009

discharge at each discharge station as well as maize yield in the agricultural HRUs. We now explicitly show the relative sensitivity of discharge (to which surface runoff contributes) to changes in all sensitive parameters. The calibration process was able to narrow the uncertainty bands by constraining the parameter ranges.

Comment 17: "The crop development growth function /water productivity: This function is not verified substantially for local conditions, although local crop parameters are used. Nor is any sensitivity analysis done on these crop parameters. In view of the importance for WP assessments and also in the following uncertainty analysis, some improved check of the validity of this function should be done. This could potentially alos improve the very large uncertainty associated with yields (Fig 2). (This uncertainty is almost unacceptable from a reality perspective as it suggests upto 100% variation in yield. . . This alone should motivate the authors to check the crop growth function in more detail)"

Response: The model is independently validated for local conditions (see Fig. 2 - evaluation period). We rephrased the sentence in the methodology to more clearly indicate this. We added the sensitivity analysis that was done for the crop parameters (see previous comment). Additionally, we analysed the response of the model to different levels of water availability, comparing the water stress during the base setup with that of the supplemental water demand scenario. It shows the model does respond to enhanced water availability. The crop yield function was also evaluated against independent maize yield data for the commercial systems (results are not shown in this paper because its objective is on smallholder agriculture). The results indicate that the model adequately responds to e.g. nutrient and water stresses (which were lower in the commercial systems) resulting in an order of magnitude higher yields in the simulations, as in the observed data. Even further checks on e.g. crop growth development, cultivar types, harvest indices, transpiration regimes etc. would be desirable. Unfortunately such data is not yet available for smallholder systems; hence a comparison is not possible at this point in time. We included a new section in the discussion on the

HESSD

6, C2413–C2422, 2009

Interactive Comment

large uncertainty of the crop growth model and on how it may potentially be reduced in the future.

Comment 18: “The HRU: The authors should indicate how many HRUs the basin consist of, and how many HRUs each sub basin consists of in Materials and Methods. Ideally, the HRUs containing smallholder farming systems (the scope of the paper) should be shown. Overall the area of these systems is fairly small in the basin.”

Response: We added details on how many sub-basins and HRUs the simulations was based on. The smallholder HRUs are indeed too small to be meaningfully displayed on a Thukela scale map. Figures 4, 6, 8 and 9 (HESSD manuscript) indicate the sub-basins within which smallholder HRUs exist (all coloured sub-basins).

Comment 19: “The manuscript tackles on one part water productivity in smallholder farming systems in the basin, and one part on the uncertainty and, implicitly, viability, of in-situ WH in the smallholder farming systems. Although both topics are relevant, the water productivity does not add any new scientific insights. In the manuscript the WP is generated as a function of modeled yield (ie by a crop growth function limited by temp, available water an available nutrients) and output actual evapotranpiration (assigned symbol ET). The ET is thus a function of the modeled crop growth (ie LAI). If the crop growth function is not well representing the local crop growth (which is not checked/validated by the authors) there is a systematic error in both parameters. The crop growth function should be discussed and analysed/ verified for local conditions further.”

Response: The crop water productivity (CWP) results are primarily included to illustrate the very low CWP in smallholder systems, giving impetus to enhance the CWP. The potential ET is a function of the climatic input data. The actual ET is limited by the crop growth function, but also considerably by the soil moisture budget and the canopy evaporation function. The proportion of rainfall ending up as ET corresponds to observations from field-scale research in the basin. We have added the comparison to the manuscript. The crop growth function is already validated for local smallholder maize yield, which show no systematic deviation (Figure 2a). Hence we do not see a systematic error in both parameters. We elaborate further on the crop growth function and its uncertainty in the discussion.

Comment 20: “The manuscript does not adequately link the analysis of WP with the following in-situ WH analysis: An obvious question is of course how much could WP be improved by the proposed in-situ WH. In addition, as the smallholder farming systems constitute a fairly small proportion of the basin, perhaps larger WP gains can be made elsewhere?”

Response: The potential impact of in situ WH on the crop water productivity (CWP) is indeed an interesting research question. Our results refer to the cropped areas of the basin only (hence “crop water productivity”). The smallholder systems constitute ca. 44% of all cropped land in the basin (cf Table 2). Hence improvements in CWP in smallholder systems may actually have a substantial impact on the CWP for the cropped areas of the basin. In order to treat the complexity of the subject adequately (i.e. more refined than assuming full potential water availability and meeting the full crop water deficit), we plan on investigating this more thoroughly in a forthcoming publication (cf. P.4938 L.8-10). We further elaborated on the link between CWP and WH in the text.

Comment 21: “Terminology: use “rainfed”; use “dry spell””

Response: We prefer “rain-fed” but have changed to “dry spell” instead of “dry-spell”.

Specific comments in article supplement to general interactive comment

P.4920, Comment 1:” OR Is WP a help for water planners/ managers rather that farmers , to manage and allocate water resources?”

Response: Yes it may be. The sentence refers to all actors influencing the course of food security, in which water managers also have a role to play.

P.4920, Comment 2: “check sentence: unclear”

Response: We rephrased it.

P.4920, Comment 3: “Low crop water productivity: is an inherent function of the crop growth function in model and not necessarily due to in situ WH?”

Response: Yes. This is results for the current conditions. We rephrased the sentence to make it clear.

P.4921 Comment 1:” this section is not really logic: the climate sets the potential transpiration rates, and crop/plant, available moisture and other factors determines actual rates. Unclear in text how the transpirational rates have anything to do with projected hungry inhabitants...”

Response: The climate, crop type, available moisture, canopy resistance etc. set the actual ET rate. Transpiration is needed in photosynthesis to produce biomass, and thus to be able to harvest any food. If one wants more food (without importing it), one needs more transpiration. Since transpirational fluxes are large, a lot of water will be used which in turn may put pressure on the water resources in water-limited areas. We rephrased the text.

P.4921 Comment 2: “Change to : a family of strategies centre on improving crop water availability (through increased infiltration i.e. in situ WH and Soil Cons strategies, and through improved water availability i.e. irrigation suppl/deficit irrig.) (the aim is firstly to increase yield, secondly to improve WP ,- not the other way around)”

Response: We rephrased it.

P.4921 Comment 3: “AND : SSA is also more than 90% rainfed with very marginal investments in irrigation”

HESSD

6, C2413–C2422, 2009

Interactive Comment

Response: Correct.

P.4921 Comment 4: “relevance of reference?? Gurtner”

Response: Gurtner et al. (2006) have done extensive research on a large set of WH technologies and describe them well (hence useful for people not so familiar with WH technologies).

P.4922 Comment 1:” Use ’partitioning’ through out”

Response: We prefer "repartitioning" since the water is already partitioned naturally. The aim with WH is to change this partitioning, hence "repartition".

P.4922 Comment 2: “not only VPD: also temp, radiance, wind... Use ’atmospheric evaporative demand”’

Response: We changed it.

P.4922 Comment 3: “Delete this section line 24-28: Gl9obally much WH containing storage , is used firstly for domestic , rather than agricultural purposes”

Response: Correct, the existing storage globally is primarily for domestic purposes. However, suitability assessments (i.e. the gist of the sentence and of Table 1) are in fact primarily focussed on agricultural water use. Hence we keep this paragraph.

P. 4923 Comment 1: “This sentence repetition of above:delete”

Response: We do not see any repetition. The preceding sections are about reliability, agricultural water demand and surface runoff, not about the uncertainty of datasets and process simulations.

P.4923 Comment 2: “define in-situ WH above (page 4921 line 20 onwards)”

Response: We defined it.

P.4924 Comment 1: “It stated semi arid above page 4923 line22”

Response: The basin climate is diverse hence this unclarity. We rephrased it.

P.4924 Comment 2: "Confusing writing: does this mean total crop area is 8%+8%+2% of basin? See also general comments separately regarding model sensitivity both for WP but also calibration when focus on a relatively small part of basin for calibration/evaluation of model performance"

Response: We rephrased the text, added a table to show the land use more clearly and a reference to the table. See above for sensitivity analysis. We do not focus only on a small part of the basin. The entire basin is calibrated against ten discharge stations on daily resolution. The crop yield and CWP are calibrated for all the cropped areas (i.e. 14.7% of the basin) but the results also only refer to all these cropped areas. We inserted a land use table.

P.4926. Comment 1: "Why this threshold?"

Response: It was based on the resolution of the HydroSHEDS dataset and on cross-checking against a field-verified drainage-network dataset. See response to Comment 18 from the general interactive comments above.

P.4926. Comment 2: "Some data appears very coarse in relation to the processes that is aimed at evaluating: in-situ WH is mainly addressing hydrol processes at maybe max 20m in this location?"

Response: True. Soil data is coarse; however land use and DEM data is of relatively high resolution. We used the best available data.

P.4926 Comment 3: "Missing word/incomplete sentence"

Response: We rephrased it.

P.4927 Comment 1: "Need to complete section with describtion of non agriculturla land extent , type, as this affects measured and modelled runoff which is used in calibration"

Response: We added a table listing all major land uses in the basin (Table 2).

P.4928 Comment 1:" Would be helpful to know something about the seasonal characteristics of the calibration and evaluation periods"

Response: The overall seasonal characteristics of the basin are given on P.4924 L.5ff. We added the annual rainfall for each year in each period in Fig. 2.

P.4931 Comment 1: "Line 1-5: this section assumes the model crop growth function is adequately responding to water deficit and implicitly) nutrient deficiency. The authors should try to verify this better for the local conditions. See additional comments."

Response: We included an analysis of what the water and nutrient stresses were for the current conditions and how they changed with the SWD scenario.

P.4931 Comment 2:" This is problematic as in many crop growth models: the low yield levels are relatively poorer modelled that higher yield levels. In this case uncertainty is 100% of actual yields for one period, and 50% of yield in the other. In such low yield systems when it is know n that 20-300kg/ha can make or break a household income, this is not sufficiently exact. The authors should at least address this issue in the Discussion: how can uncertainty in yield prediction be substantially improved?"

Response: We included a new section in the discussion on the large uncertainty of the crop growth model and on how it may potentially be reduced in the future (see above Comment 17 in general interactive comments).

P.4933 Comment 1: "Again, how can this be improved in the model? In the data?"

Response: See previous comment.

P.4934 Comment 1: "See separate comment regarding the crop growth function and its response to water deficit"

Response: See response to Comment 1 on P.4931 above.

P.4935 Comment 1: "p4934 line 25-4935 line 9: this section and Fig9 is redundant. Would be more relevant to compare with any of the products mentioned in Table1?

which sues similar set of landuse. Possibly move section to supplement section”

Response: We removed the section as suggested.

P.4938 Comment 1: “Investors?”

Response: We prefer “decision-makers” since it is a wider concept (including “investors”).

P.4939 Comment 1: “This is addeing new knowledge, but it is unlikely that for example commercial sectors or applied users would do this lengthy complex modelling exercise. in addition, this particular basin is very data rich, compared to most SSA conditions: what does this work add in terms of usefulness for other locations? (i.e. generic knowledge generated from this research work)”

Response: We agree. We expanded the discussion to include some suggestions on the generic benefit of this study.

P.4951 Comment 1: “this appears to agree fairly well: a table summarising cumulated discharge over calibration/evaluation period would add valyue: can indicate drfting...”

Response: We added a table with the accumulated discharge for each period. Note the observed data contains missing flow records. Hence the accumulated discharge over each period can only be made for those days with observed data. The number of days missing are different from each station and each period, hence they are not directly comparable. But it is possible to compare each station in each period with the corresponding uncertainty band.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 4919, 2009.

HESSD

6, C2413–C2422, 2009

Interactive Comment