Hydrol. Earth Syst. Sci. Discuss., 6, C2396–C2403, 2009
www.hydrol-earth-syst-sci-discuss.net/6/C2396/2009/

Hydrology and Earth System Sciences Discussions

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 comments

Comments: "The objective of this study is to estimate water availability for in situ water harvesting (WH) and supplemental water demands (SWD) in smallholder agriculture in the large Thukela River Basin (29000 km2), South Africa by incorporating spatiotemporal process dynamics governing runoff generation and crop water demands and uncer-

tainty considerations. The objective of the paper is of international interest with wide application in water scarce environments (dry areas). The agro-hydrological model SWAT (Soil and Water Assessment Tool) was simply calibrated and evaluated with the SUFI-2 algorithm using available crop yield and discharge data. The confrontation of water availability against supplemental water demand resulted in the delineation of in situ WH for smallholder systems sites with various levels of reliability. The paper is written in well and clear English. The bibliography is complete and of international broad. However, the authors failed in presenting well the model parameters and justifying the choices and assumptions. The interpretation and discussion of the results lack also soundness especially when related to the analysis of the hydrological processes."

Response: We thank referee #1 for helpful comments on how to improve our manuscript. We added a table listing the calibrated parameters and the sensitivity of the objective functions to their alteration (Table 4). The justification for choosing these parameters was primarily based on the sensitivity of the output. Some parameters were also included, although not significantly affecting the objective functions, because they improved the hydrograph patterns relative to observed data. The cumulative changes (particularly in response to Comment 18, 20, 24, and 25) address the concern regarding the analysis of the hydrological processes.

Specific comments

Comment 1: "P.4920/Abstract: Clarify whether it is for potential or for already implemented WH. Maize should be announced as the dominant/used/studied crop. Indicate the study period (years) and the rainfall amount. Add values of the model evaluation indices."

Response: The abstract was revised according to these comments and we have added some additional quantitative information on the evaluation indices.

Comment 2: "P.4924 L:1-7: Provide more details (average, min, max, etc) on precipitation and temperature in the study area as well as the soil characteristics (texture,

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depth, OM, water holding capacity, etc)."

Response: We added this to the study-area description.

Comment 3: "P.4924 L:25-27: 'There are ongoing efforts to promote WH ..'. You need to specify the areas already (if any) covered by WH systems and the targeted objectives. More information is requested on the types of WH techniques."

Response: We added more specifications.

Comment 4: "P.4925 L:3-8: Elaborate more on the choice of the SWAT model and especially it is successful applications in similar environments (dry areas)."

Response: We included text regarding SWATs performance in a variety of climatic conditions and a reference to the study by Van Liew and Garbrecht (2003) aimed at testing this ability.

Comment 5: "P.4925 L:20-22: The paragraph 'Potential evapotranspiration . . ...transpiration' can be moved to the section 2.2.2.."

Response: Section P.4925 L:20-22 moved to P.4926 L:26

Comment 6: "P.4926 L:3: Why the subbasin threshold area was set to 2025 ha ?"

Response: It was based on the resolution of the HydroSHEDS dataset and on cross-checking against a field-verified drainage-network dataset. Clarified in the text.

Comment 7: "P.4926 L:6-11: How homogenization to 10 m resolution was done with two maps of very coarse scales ?"

Response: Homogenisation was done through resampling. We have clarified this in the text. The reason was in order not to loose crop field resolution in the process of combining of the two datasets (The NLC dataset originate from Landsat Enhanced Thematic Mapper Plus data with 30 m resolution and the CFB dataset from SPOT 5 data with 10 m resolution).

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Comment 8: “P.4926 L:16: Indicate the length of climatic records.”

Response: The length of the climatic record is indicated on P.4926 L.17.

Comment 9: “P.4926 L:27-29: Be more specific: ‘available quantitative data on water management’, ‘minor amount of missing values’.”

Response: The available data constituted the reservoir outflow data and the water transfer data. The average number of missing data points for all included reservoirs was 3%. We changed the text accordingly.

Comment 10: “P.4927 L:24-25: The types of non crop land covers (which represent 84% of the basin area) need to be specified and the use of default parameters requires justification.”

Response: We added a table to show all major land cover classes. The default parameters were only used in model setup. All parameters that were sensitive to model outputs (i.e. to discharge and crop yields) were subsequently calibrated to the local conditions, such that the results were not dependant on the assumption of default parameter values.

Comment 11: “P.4927 L:27: Reasons for the selection of SUFI-2”

Response: We have added a sentence to explain our choice.

Comment 12: “P.4928 L:12-14: Why the first observation period (Jan1st, 97 to Dec 31, 2001) was used for validation not calibration as it is done usually ?.”

Response: Our aim was to use the best available data for the calibration. The more recent data was the most reliable and contained the least amount of missing data points. Hence this choice.

Comment 13: “P.4929: Why the evaluation index of discharge is different from that of crop yield ?.”

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Response: For crop yield our aim was to assess the model's capacity to simulate yield over the entire time period. This is better captured by RMSE than by Φ. For discharge, we were additionally interested in the model's capacity to capture the time-resolved dynamics, which is better captured by Φ than by RMSE. The limited amount of data available for smallholder crop yield rendered a time-resolved crop yield analysis unfeasible.

Comment 14: "P. 4931 L:1: It is hardly difficult to consider that 100% of generated runoff can be considered available for WH."

Response: This is correct. We emphasize this point in the discussion (P.4937 L.18 ff.). We use the entire amount of surface runoff in this paper to study the reliability at the upper boundary of the potential water availability for in situ WH (excluding evaporation minimisation techniques). This in order to strengthen the reliability message: even if one considers all runoff as available for in situ WH, the reliability is still low.

Comment 15: "P.4931 L:10: How the crop water deficit is known ? Any relations with the soil water content ? What level of water stress is accepted ?"

Response: The energy availability (climate input) set the preconditions for potential biomass production. Biomass production induces plant water and nutrient demands which cause water and nutrient uptake from the soil in relation to their availability (as simulated through their respective soil budgets). In a situation of limited soil water availability the plant can only withdraw less water than the full/potential biomass production would demand. The difference between the potential and the actual amount of transpiration is the crop water deficit. The water stress level used for the peak SWD calculations was >0 (i.e. as soon as there was water stress). The text has been changed to make this point clear.

Comment 16: "P.4932 L:20: Any effects of exceptionally high runoff events ?"

Response: Overall this effect is minor. 83% of flows $\geq$ the 95% percentile of observa-

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tions were within the 95PPU and only 17% outside. Text adapted accordingly.

Comment 17: "P.4932: L:28: Specify 'inadequate simulation of soil processes'" (P.C1962 L.20)

Response: We specified it in the revised text.

Comment 18: "P.4933 Section3.2: In this section, the interpretation of the results needs to include the soil characteristics, the climate and crop parameters, etc."

Response: We included this.

Comment 19:"P.4934 L:10-14: Specify the % of each main water balance component: rainfall, infiltration, ET, outflow, etc."

Response: We added a table with the major components of the basin average water balance (Table 7).

Comment 20: "P.4934 L:15-24: In this section and as above, the interpretation of the results needs to be related to soil characteristics, climate and crop parameters, etc."

Response: We elaborated more on this point.

Comment 21, 22, 23 and 33: "P.4934 L:26: Provide complete name of the acronym ACRU.", "P.4934 L:27: Precise the 50-year time period.", "P.4935 L:6: How the altered land use has an effect only for MAR>300 mm ?", and "P.4957 Fig. 7: Specify the period."

Response: On recommendation from referee #3 we removed this section (P.4934 L.25 to P.4935 L.9) and the associated figure (Fig. 7). Thus these comments are no longer applicable.

Comment 24:" P.4935 Section3.4: In this section and as above, the interpretation of the results needs to be related to soil characteristics, climate and crop parameters, etc."

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Response: We elaborated more on this in the revised manuscript.

Comment 25: "P.4935 Section3.5: In this section and as above, the interpretation of the results needs to be related to soil characteristics, climate and crop parameters, etc."

Response: Done.

Comment 26: "P.4936 L:3: Why Spearman correlation coefficient is introduced only at this level and not at the methodology section ?."

Response: An omission on our part. We added this to the methodology.

Comment 27:" P.4936 L:19: '7000 to 9000 ha ' (add percentage)"

Response: We added it.

Comment 28: "P.4936 Section 4.1: This section lacks comparison with other studies in similar environments."

Response: We added comparison.

Comment 29: "P.4937 Section 4.2: The same remark as above."

Response: We compare the reliability with Kumar et al. (2006) on P.4938 L.29. We also compare the risk implications of low water availability with de Winnaar et al. (2007) on P.4934 L.24. Additional comparison was made to Hatibu et al. (2000).

Comment 30: "P. 4949 Fig.1: The town labels are not clear on the map."

Response: We clarified the town labels.

Comment 31: "P.4950 Fig.2.: The box-and-whisker plots are not clear at all. How the observed maize yield is stable all over the calibration period ?. It is recommended to add the annual rainfall."

Response: We removed the box-and-whisker plots and put the main information in the

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text. The observed maize yield originate from the Crop Estimate Committee of South Africa. Their yield is not constant but the variation is small compared with the range of the y-axis (which is necessary given the uncertainty range). We added the annual rainfall.

Comment 32:" P.4951 Fig.3.: The plots are not clear and need to be redrawn."

Response: We redrew the figure and included fewer but larger graphs (for three stations).

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