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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Impact of LUCC on Streamflow using the SWAT Model over the Wei River Basin on the Loess Plateau of China" *by* Hong Wang and Fubao Sun

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This paper examines the hydrological effect of land conversion from agricultural land to forest in a sub-catchment of the Yellow River. The SWAT model is calibrated with daily hydro-meteorological data over the period 1960-1969 and validated over the period 1970-1979, using the same set of parameter values characterizing soils and land-use in 1980. Two land-use maps of the catchment are available for this study: one corresponding to the year 1980 (used for the model calibration) and one for the 2005 (used for simulation).

The calibrated SWAT model is then run twice to simulate flow using 1980-2009 meteorological input, the first time with land-use parameters of year 1980, and the second

time with land-use parameters of years 2005. The hydrological impact of land-use changes that occurred between 1980 and 2005 is quantified by comparing the two simulated flow time series. Finally, five land-use scenarios are defined, corresponding to a gradual increase in the percentage of agricultural lands converted to forest, and their effects on flow are simulated with the SWAT model.

Main comments:

This is an interesting topic but the approach needs significant improvement in order to provide evidence of the actual hydrological effects of the land-use and land-cover changes that occurred in the catchment. I recommend major revisions.

First of all, I am questioning the significance of the hydrological changes that actually occurred in the catchment over the studied period. Although figure 1 indicates that forested areas increased by about 65.104 hm2 (unusual unit used on the Y axis), which is equivalent to 14% of the upper catchment area, figure 3 inconsistently shows that forested area increased by only 0.81% (line 137) over the same period (1980 to 2005). How to explain this difference? If we rely on figure 2 (which is likely the most reliable source), we can expect minor influence of forestation on the basin hydrology.

The main issue of this paper is that all the demonstration relies on simulated flows only. Flow simulated over the period 1980-2009 with land-use from 1980 should be compared to actual flow recorded over the period 1980-2009. Another issue is the implicitly presumed stability of the catchment behaviour over each of the 2 periods 1960-79 and 1980-2009. This hypothesis should be further justified. Before modelling, the authors should start their assessment by analyzing actual rainfall and flow data. A graphic showing annual flow, rainfall (both in mm) and runoff coefficients in each of the 3 nested catchments and intermediary catchments (e.g. the colored areas in figure 2) would provide a first assessment of the possible effects of the land-use changes (as done in Lacombe et al. (2008)). A statistical assessment quantifying change and/or trend significance is also missing (cf. Lacombe et al. (2016) for an example).

There is an overall lack of clarity in the writing. The methods used should be explained in more details and with more precision. Figure 1 shows 4 types of treatments for water and soil conservation that occurred in the study area: forestation, terraces, grass and dam. The hydrological impact assessment focuses exclusively on forestation while the 3 others are completely ignored in the analysis. They certainly have altered river flows too. How to account for their effect in the SWAT model? The maps of the study area (figures 2 and 3) do not show where these technics have been implemented.

Splitting the section "Results and discussion" into two distinctive sections "Results" and "Discussion" would certainly help the authors clarifying their scientific demonstration. As it stands, in many places, actual results are juxtaposed with results of previous research which are not referenced.

Detailed comments:

The title should be improved. Currently, it says that LUCC is impacted by the SWAT model.

Abstract: in line 29, it is mentioned that SWAT is applied to the upper and middle reach of the Wei River Basin. It is not clear what is the role of the hydrological station at the outlet of the lower reach.

Introduction

Line 46: a/ the location of the Grain for Green project is missing. b/ Which trees are used for the reforestation? This information is important because, depending on the trees (e.g. deciduous or not), their effect on seasonal flow may be different. c/ the mode of forestation is also primordial when assessing hydrological impacts. For example, natural forest regrowth or tree plantation can have opposite hydrological effects, depending on how the soil is altered. (cf. Lacombe et al. 2016). The authors should provide more details on the type of forestation.

Lines 62-65 do not provide much information, saying that streamflow can increase

whether the vegetation increases or decreases. Too many references here, should be split in two groups (case studies with vegetation increase and case studies with vegetation decrease).

Line 73-75. I don't think that catchment size is the primary control influencing the direction of flow change following land-use change. It is more a question of trade-off between modified infiltration rate and evapotranspiration rate which depends on soil structure, surface properties, depth, slope, vegetation species, etc. . .

Lines 79-82. The explanation lacks clarity. Again, latitude may indirectly control the hydrological impact of land-use change, but this is certainly not the primary key player.

Line 89: it is not clear if 43% corresponds to the total treated area included in the Wei Basin or if 43% of treated areas corresponds to afforestation.

Line 90: This statement should be supported by a figure showing the time series of actual annual flow (cf. main comments).

Lines 91-92: "streamflow" and "observed annual streamflow". Are you referring to the same variable? Please keep using the same wording when referring to the same variable.

Lines 93-95. Description of geology should be included in the section "study area".

Line 96: "And that drying layer is in great water deficit". Why? Reference required.

Lines 95 to 103: The explanations of the contrasting hydrological behaviours between the "earth-rock mountain landscape" and the Loess Plateau are not clear and not convincing. You did not mention the possible role of slope which is very different between the two types of landscape.

Study Area

Lines 117-118: need to explain what the units provided define exactly.

Line 132: MODIS ?

Line 134: cannot see the six types of LUCC in figure 3.

Lines 136-137: Forest area increased by 0.81% only. It is hardly believable that the hydrological impact quantified later (line 270), (annual average reduction of 94 million m3) was caused by this very minor change.

Line 141: unlike what is written, the soil characteristics are not indicated on the map, (only the names of the soil types are provided).

Line 145: meaning of HRUs ?

Lines 154, 239 and 264-265: avoid “and so on”.

Line 160: cf. advices provided in my main comments.

Line 179: need to provide much more information on the input data used to run SWAT.

Lines 185-186: what is an “extraction threshold”?

Line 190: if subdivided into 1 HRU, then it is not subdivided. Please clarify.

Page 11: many parameters and initial values used to calibrate the SWAT model were issued from previous research and experiments (e.g. lines 219: “derived from simulated rainfall experiments”, 228: “We have done some research”, 230: “Based on the experiments”, 234: “were gotten based on experiments”). No references and no explanations are provided. We need more details to understand what has been done.

Lines 237: It is not clear how the authors have accounted for the “management operation of forest” which affect “leaf area index [. . .], plant biomass [. . .], age of trees”. Need to provide some explanations here. Which management operations are accounted in the model and how do they affect the variables listed here?

Results and discussions

Line 253: It is not clear if the model efficiencies provided correspond to an average for

each hydrological unit or for the whole basin.

Lines 257, 258: unlike what is written, the trend is not obvious in fig. 6. It would be clearer to redraw the figure at the monthly and annual time steps to visualize possible trends over years.

Line 269: it is not clear what is the 20-year period referred here. Calibration and validation periods are 10 years long and simulation period include 30 years. Further explanations are required. Line 270: there are 3 problems here. 1/ it is not clear in which catchment the hydrological change (annual average reduction of 94 million m3) was assessed, upper or middle ?. b/ this hydrological change should be translated into millimeters of runoff reduction to assess its magnitude and significance. c/ the text indicates that this change is caused by forestation. Indeed, it only reflects the change in the model parameters between the calibration/validation and the simulation periods. But, as already indicated, it does not reflect the actual changes that occurred in the catchment.

Line 273: reference required when referring to previous experiments.

Lines 278-279: “30-year average of the streamflow for forest and agricultural land were taken”. Please explain what was done exactly here. Are you referring to the two sets of simulated flow described in lines 263-267? or different hydrological units with agricultural land or forest cover for a given period?

Lines 291-294. This paragraph is about method and should be moved in the appropriate section. It is referring to 3 regions. Which ones? Three different approaches are described to define the LUCC scenarios but the results of each approach are not presented. It seems that figures 8, 10 and 12 only present results for approach 1.

Line 306: the authors indicate that the actual change in forest cover calculated using the land-use maps displayed in figure 3 (0.8% increase) would lead to less than 1% change in streamflow. I agree with this realistic statement but: is it consistent with the

hydrological change quantified in line 270?

Lines 314-325. the authors explain differences in hydrological behaviour of the Loess Plateau and earth-rock mountain, based on other publications, but this paragraph is not linked to the result of the study. The authors need to evidence how these distinctive hydrological behaviours influence their results.

Figures

Fig. 1: Areas under different treatments are expressed in 104 hm2 (i.e. squared hectometers?). This is an atypical unit which is different from the unit used for the study area in the text (104 km2). All areas should be provided in same unit to allow easier comparison. It would be clearer to provide the percentage area so that we anticipate the possible effect of the land treatment on the catchment hydrology.

Fig.2. What is the meaning of all small numbers written on the map of the study area? If they correspond to hydrological units, it is surprising to see numbers in the downstream part which is not included in the study area.

Fig. 6. The scale on the X axis is too big: we cannot see the details in the daily flow variations and in the matching between observed and simulated flow. The figure should be bigger or all panels (calibration and verification should be put in the same column to allow larger size.

Fig. 9: What is the meaning of "corresponding proportional change rate"?

References:

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