

## ***Interactive comment on “Evaluating the impacts of land use changes on hydrologic responses in the agricultural regions of Michigan and Wisconsin” by A. P. Nejadhashemi et al.***

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We greatly appreciate the positive comments from the referee #2. We have addressed all the questions and concerns. In the following, we include the original comments along with our responses.

Comments for the Author \*General comments\* Comment: The authors applied SWAT model to simulate the hydrological fluxes at 9 selected watersheds in the Great Lakes region based on two land use conditions: mid-1800 and current condition. The changes of hydrological responses were then evaluated at three scales: 1) subbasin-level by

analysing the correlation between percent of land use conversion and hydrological variables; 2) watershed-level showing the percent changes of hydrological variables in each watershed and 3) basin level for the whole study area.

Comment: In general, it is a good practice to evaluate the impact of land use changes in the Great Lakes region as a case study. However, Mao and Cherkauer (2009) have already examined the effects of land use change on hydrologic responses in the similar regions also based on the pre-settlement condition, and the authors didn't highlight their own novel concepts, data or tools compared to this previous study.

Response: Thank you for your comment. Previous studies such as Mao and Cherkauer (2009) used coarse distributed large-scale hydrology model with spatial resolution of 10-15 km grids while in our study the resolution is a hundred times finer. In addition, Mao and Cherkauer (2009) only considered five classes of land use, ignoring urban landuse, while we used up to 21 classes of land use, including three urban classes. Mao and Cherkauer (2009) calibrated their model only based on five HUC6 watersheds for the entire great lakes basin, whereas in our study the model was calibrated for all watersheds within the study area (8 HUC6 watersheds). The study area considered by Mao and Cherkauer (2009) is four times larger than our study area. However, in our research, the study area is focused on regions of intense agricultural production. In our study, a comprehensive sensitivity analysis was performed for each watershed based on current and pre-settlement land use scenarios, while Mao and Cherkauer (2009) performed sensitivity analysis on a single cell. We studied the impact of land use change in three levels (subbasins, watersheds, basins), while Mao and Cherkauer looked at the regional scale impacts.

We added the following sentence to the introduction section to address your comments. "The aim of this paper is to examine the effects of land use change on hydrologic fluxes at both local and regional scales, under finer and more detailed resolution than existing studies, such as Mao and Cherkauer (2009)".

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Comment: Secondly, there is an important factor missing in the assumptions of this study. The soil texture data is an important component in SWAT modelling, but such data is not described in the "Data source" part. In addition, as the authors mentioned at the page.

Response: Thank you for your comment. The following sentence was added to the manuscript under "Data Source" section to describe the source of the soil data used. "In addition, the Natural Resources Conservation Service's State Soil Geographic (STATSGO) database was used in the model".

Comment:3433 line 1-6: the soil deteriorates along with the land use change, the soil texture in about 150 years may not remain the same. It is not clear from the present paper whether the authors considered the impacts of the changing soils

Response: Thank you for your comment. We did not consider the soil deterioration in the past 150 years since the data is not available. In addition, the following paragraph was removed from the manuscript to clarify that this was not part of the study. "As land use changes from forest to agriculture, the soil structure generally deteriorates. This deterioration is evidenced by reduced pore space, increased bulk density, increased compaction, reduced content of water-stable aggregates, and reduced rates of infiltration. Soil 5 deterioration effects surface water runoff, stream flow, and sedimentation (Carmen, 1954)."

Comment: Thirdly, the description of the results and conclusions is very general. More quantitative results should also be written in the text.

Response: Thank you for your comment. Authors goal in the conclusion section is explore the significance of the results of the work and not repeat the quantitative values described in details in the "Results and Discussion" section. However, the conclusion section was rewritten to better address the concern. Please see the paragraph below:

"At the subbasin level, based on the results of the statistical analysis, several signifi-

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cant correlations were found between the percentage of landuse change and both absolute and relative differences in hydrological behaviors. Of all land use conversions, only mixed forest to urban and agricultural lands showed significant correlations for all hydrological variables. Concerning watershed scale impacts of land use changes, a Wilcoxon Signed Rank Sum, S test confirmed that the long-term average fluxes under the current and pre-settlement scenarios were not the same. Similar results were reported in many studies such as Matheussen et al, 2000; Andreassian, 2004; Brown et al., 2005; Coe et al., 2009. Overall, an increase in evapotranspiration (up to 16.5%) and surface runoff (up to 93.9%) contribution to stream flow, decrease in recharge to aquifers (up to -51.5%) and baseflow (up to -50.1%), and mixed impacts on water yield were detected (-21.5% to 24.6%). Finally, at the basin-level, modest changes in evapotranspiration and water yield, significant increases (65% of study area) in overland flow generation, and significant decreases (70% of the study area) in recharge, baseflow, and lateral subsurface flow in the majority of the basin were observed.”

Comment: Fourthly, as the authors mentioned in the abstract " In addition, the study can help in quantifying the potential impacts of future projected changes in land use in order to mitigate the negative impacts of these changes on goods and services of value to society", I would expect more discussion on this point. However this sentence is just repeated at the end of the conclusion. The usefulness of the results should be discussed further.

Response: Thank you for your comment. The following sentences were added to the “Conclusion” section to address your concern. “The results of this study can be used in quantifying the potential impacts of future projected changes in land use in order to mitigate the negative impacts of these changes. For example, concerns with energy and a shift towards second generation biofuel production derived from lignocellulosic crops has the potential for large scale landuse conversion within the region, which can have significant impacts on hydrologic components (Love and Nejadhashemi, 2011). In addition, the results of this study can provide insights into future urbanizations impacts

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of water resources.”

Comment: Abstract: the summary of the results in the abstract is too general. Please describe it more quantitatively.

Responses: Thank you for your comment. The abstract was rewritten as follow:

“Hydrologic fluxes in the Great Lakes region have been altered relative to pre-settlement conditions in response to major land use changes during the past 150 years. One of the goals of the present work is to develop a baseline scenario relative to which the impacts of land use changes on hydrological and environmental processes can be evaluated. In addition, the study can help in quantifying the potential impacts of future projected changes in land use in order to mitigate the negative impacts of these changes specially in regard to shift towards second generation bioenergy crop production derived from lignocellulosic crops and urbanizations. The present study explores the relationship between land use changes and hydrologic indicators within the agricultural regions of Michigan and Wisconsin. Two sets of land use data, the Circa 1800 County Base and the 2001 National Land Cover Dataset, were used to setup the Soil and Water Assessment Tool (SWAT) model. First, sensitivity analyses were performed both based on pre-settlement and current land use scenarios. Results showed that parameter sensitivity analysis may not always explain how the variation in model output can be attributed to different sources of variation in the model input. Therefore, attention should be taken to determine the true importance of sensitive parameters by considering their placement in model algorithms. Then, the model was calibrated against measured daily stream flow data obtained from eight United States Geological Survey gauging stations. The impacts of land use changes were studied at three scales: Subbasin-level, watershed-level, and basin-level. At the subbasin level, most of the hydrologic behavior can be described by percent change in land cover. However, the trend was more apparent for landuse conversion from mixed forest to urban and agriculture lands than other landuse conversions. At the watershed scale, significant differences were observed based on the long-term average hydrologic fluxes under the

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current and pre-settlement scenarios. In addition, an increase in evapotranspiration (up to 16.5%) and surface runoff (up to 93.9%) contribution to stream flow, decrease in recharge to aquifers (up to -51.5%) and baseflow (up to -50.1%), and mixed impacts on water yield were detected (-21.5% to 24.6%). However, at the basin-level, more than 70% of the study area experienced decreased in lateral subsurface flow and recharge to aquifers, while 65% of the area experienced increased overland flow and minor changes in evapotranspiration and water yield.”

Comment: Introduction: the third paragraph, which discussed the impacts of land use and climate change on hydrological processes, is quite confusing. The authors firstly pointed out that the climate change is a key driver behind increasing stream flows in the Midwest, and then found that there was an consistent trend of increasing annual stream discharges in the studied area. From this paragraph, it is expected that the authors should evaluate the climate impact in this region but not the land use changes.

Response: Thank you for your comment. Authors agree that having the following sentence can be misleading; therefore, it was removed from the manuscript. “The recognition that climate change is a key driver behind increasing stream flows in the Midwest also means increased susceptibility to nutrient losses from agricultural landscapes contributing to degradation in water quality and Gulf of Mexico hypoxia.”

Comment: Page 3424 line 27: "(Hundecha and Ba'rdossy, 2004)" This in-text citation has no use.

Response: Thank you for your comment. The period before the "(Hundecha and Ba'rdossy, 2004)" was removed to show it as a reference to the previous sentence.

Comment: Page 3426 line 1: "... which includes 41 HUC 8 digit watersheds (Fig. 1) ...". In Figure 1, only 9 watersheds can be found, and the whole paper is only focused on these 9 watersheds. Better to remove this sentence. In addition, it will be clearer for readers who are not familiar with the study area to know the location of the watersheds if the authors mark "Michigan" and "Wisconsin" in Figure 1.

Response: Thank you for your comment. The sentence was revised and this section was removed “includes 41 HUC 8 digit watersheds”. In addition, "Michigan" and "Wisconsin" were added to Figure 1.

Comment: Page 3428: When describing different methods of estimating potential evapotranspiration and water routing included in SWAT, the method which was used in this study should also be pointed out.

Response: Thank you for your comment. The following sentence was added to the manuscript to address your concern. “However, since observed PET values were not available, daily PET values were estimated using the Penman-Monteith method.”

Comment: Page 3430: 2.3.2 Gauging station. In this section only 8 gauging stations are listed for 9 watersheds. Please explain why the gauging station 04087000 represents two watersheds.

Response: Thank you for your comment. The following sentence was added to the manuscript (gauging stations section), “Due to similarity in physiographic and climatologic characteristics of watersheds 040400 and 040301, only one gauging station (04087000) was used for model calibration.”

Comment: Page 3431 line 20-28 and Page 3432 1-3: it is easier to follow this paragraph to list the calibration parameters first and then explain which parameters are identified from the sensitive analysis and which are not. The reasons of choosing the calibration parameters can be explained afterwards.

Response: Thank you for your comment. The following paragraph was revised according to your recommendation. “The following parameters were used for the model calibrations in different watersheds: Alpha\_Bf (baseflow recession constant), Cn2 (moisture condition II curve number), EPCO (plant uptake compensation factor), ESCO (soil evaporation compensation coefficient), Rchrg\_Dp (deep aquifer percolation fraction), Surlag (surface runoff lag coefficient), TIMP (snow coefficient lag factor). Some pa-

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rameters identified as sensitive (Sol\_Z, Sol\_Awc, Canmx, Gwqmn, Ch\_K2) were not modified during calibration, while others that were not identified during sensitivity analysis were modified during calibration. Parameters that were not identified as sensitive but used in calibration were applied to match the model with naturally occurring processes in the watershed. Additionally, parameters not identified as sensitive in the sensitivity analysis must be adjusted due to error observed in predicted variables. Parameters chosen other than those identified by the sensitivity analysis were based on calibration parameters identified in other published results (White and Chaubey, 2005).”

Comment: Page 3433 line 3-6: I only understood the two criteria (mean and median) when I read the Table 3a and 3b. The meaning of these criteria should be described clearer in the text.

Response: Thank you for your comment. In order to address this concern the following sentences were added to section 3.1 “Two criteria (mean ranking and median ranking for each watershed parameter) were selected to identify the most influential parameters, which affect daily flow rates. Mean and median were calculated for the top 15 parameters based on their position in the sensitivity analysis ranking table. In the case that the mean of two watershed parameters’rankings are the same, the median value was used in determining the overall ranking.”

Comment: Page 3437 line 23: Di Luzio and Arnold (2004) focused on the calibration of hourly stream runoff, so it is not appropriate to use this example here. The criterion  $E=0.2$  indicating a satisfactory model is too low for me. In fact, the statistical results in Table 4 show a good performance of SWAT model in simulating the daily discharges in 5 of 8 watersheds ( $E>0.7$ ). The authors should explain why the river discharges cannot be well reproduced in the other watersheds by SWAT.

Response: Thank you for noticing these errors. Di Luzio and Arnold (2004) was removed from the list of examples and the satisfactory model performances on daily basis was updated to  $ENS \geq 0.40$ . As it was described in the paper “Moriasi et al.

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(2007) developed general evaluation guidelines based on a model performance rating. Based on these guidelines, a model performance can be evaluated as “satisfactory” for a monthly time step series if  $ENS > 0.50$ .”. Further calculations on a monthly basis showed that for all studied watersheds the model performed satisfactory according to Moriasi et al. (2007) with an  $ENS > 0.50$ ). Therefore, all eight watersheds were calibrated satisfactory. The following paragraph were revised to further address this concern.

“In general, shorter time steps have poorer model simulations than longer time steps (Moriasi et al., 2007). Performance ratings presented above for ENS statistics are for a monthly time steps and must be modified for a daily time step to be applicable in this study. In order to do so, a series of studies on SWAT model performance on daily basis were reviewed. For example Benham et al. (2006) ENS of 0.21 and Coffey et al. (2004) reported ENS of 0.15 for satisfactory SWAT calibration. Based on the above studies, a conservative criterion was considered to evaluate satisfactory model performances on daily basis:  $ENS \geq 0.40$ . Further calculations on a monthly basis showed that for all studied watersheds the model performed satisfactory according to Moriasi et al. (2007) with an  $ENS > 0.50$ .”

Comment: Page 3440: at the end of the section 3.3, a short summary is needed to describe the general findings from the both methods.

Response: Thank you for your comment. The following sentences were added at the end of section 3.3.

“In general, for all types of landuse conversion, significant changes in one or more hydrological variables were observed under both Spearman and Hoeffding’s D methods. However, all hydrological variables were significantly altered by landuse conversion from mixed forest to urban and agriculture lands based on the results of the above methods.”

Comment: Page 3441 line 6: this paragraph should start with a short description of the

result in Table 7, then discuss the watershed 040900.

Response: Thank you for your comment. The following sentence was added to the start of the paragraph. "Overall, consistent decreases in recharge and baseflow and increases in surface runoff and evapotranspiration were observed, while water yield showed mixed results (Table 6)."

Comment: Page 3441 line 16: " an overall surface runoff pattern is also presented in Table 6". Should it be Table 7?

Response: Thank you for your comment. It was revised.

Comment: Reference: the authors should check the reference list more carefully. There are more than 15 references which are not cited in the text but listed here. In addition, Chow et al., 1998, Copeland et al. 1996 and Wang et al., 2007 are missing.

Response: Thank you for your comment. All references were checked and revised.

Comment: Figure 1: one inset map indicating the location of the study area in the USA should be added.

Response: Thank you for your comment. Figure 1 was revised to include the study area in the USA.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/8/C1692/2011/hessd-8-C1692-2011-supplement.pdf>

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