

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.

Anonymous Referee #2

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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.

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

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

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

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.

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

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

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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.

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.

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.

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.

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

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.

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

Page 3441 line 6: this paragraph should start with a short description of the result in Table 7, then discuss the watershed 040900.

Page 3441 line 16: " an overall surface runoff pattern is also presented in Table 6".

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Should it be Table 7?

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.

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

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