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Hydrology and Earth System Sciences Discussions



# *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.

**A. P. Nejadhashemi et al.**

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We greatly appreciate the positive comments from the referee #1. 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: This paper reports a hydrological modelling study of basins in Michigan and Wisconsin, looking at differences in simulated response under historic and recent land use/ land cover. The overall aim is interesting, and the use of a model is necessary to reconstruct the historical conditions.



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The model, however, cannot be considered reliable, and no attempt has been made to critically review the applicability of the model to the problem or the uncertainty in the results. For example, we do not know what the significance of the modelled changes are given the model uncertainty (something that could quite easily be explored). The method of calibration and the assumptions employed in it are unclear, and the use of correlation analysis (as I understand it has been done) seems flawed. There is great scope for a interesting and important paper here, but I think more thought needs to be given to critically investigating the applicability of the model.

Response: Thank you for your comment. Authors tried to address your comments and concerns in the following sections.

Comment: P3423, Line 21. Is ET defined somewhere?

Response: Thank you for your comments. The definition for ET was added in the "Introduction" section. The new sentence reads "They examined the changes in annual average fluxes of evapotranspiration (ET), total runoff, soil moisture and snow water equivalent (SWE) between current and pre-settlement land uses as well as the geographic shifting of center of gravity for each vegetation class."

Comment: P3424, 21. "The recognition that climate change is a key driver behind increasing stream flows in the Midwest also means increased susceptibility to nutrient losses..." Not clear what the link between the recognition and the susceptibility: sentence needs re-written

Response: Thank you for your comment. The sentence was removed since the scope of this paper is mainly water quantity effects of land use change.

Comment: P3425, 2. "Field data and experiments have the potential to demonstrate the consequences of land use change, but modelling studies are more likely to reveal the key mechanisms (Li et al., 2007)" This seems confused: modelling cannot reveal anything without suitable field data to support the models; and field data and



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experiments cannot demonstrate consequences of change unless there is some kind of statistical or simulation model linking the data to land use. The sentence should be re-written.

Response: Thank you for your comment. Authors' goal in the "introduction" section is to demonstrate different point of views about the subject matter. Overall, authors believe both fieldwork and modeling have potential to reveal mechanisms while only models reveal consequences on a larger scale. In addition, models are built based on our understanding of natural system and models are widely used to predict future climate, landuse, etc. scenarios where suitable field data are not available. What is presented here is the Li et al. (2007) point of view that is published in the Journal of Hydrology (Li, K.Y., Coe, M.T., Ramankutty, R., De Jong, R., 2007. Modeling the hydrological impact of land-use change in West Africa. Journal of Hydrology 337, 258-268." (please refer to page 2, last paragraph on left).

Comment: P3425, 4. "Studies regarding hydrologic sensitivity assessments of current and historic land use data at the large scale have not been conducted" This is not correct.

Response: Thank you for your comment. The sentence was revised in the "Introduction" section. "Few studies regarding hydrologic sensitivity assessments of current and historic land use data at the large scale have been conducted". Meanwhile in our extensive literature review we were not able to locate any similar studies regarding hydrologic sensitivity assessments of current and historic land use data at the large scale. Authors would greatly appreciate if you can provide any references you are aware of on this topic.

Comment: P3425, 9. "The aim of this paper is to use a comprehensive approach..." I don't know any modelling research that uses a 'comprehensive approach' so I very much doubt the authors will meet this aim.

Response: Thank you for your comment. The term 'comprehensive approach' was



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removed from the “Introduction” section. The new sentence reads “The aim of this paper is to examine the effects of land use change on hydrologic fluxes at both local and regional scales”.

Comment: P3425, 26. “040802 and Saginaw.” What does this mean? Is 040802 the same as Saginaw?

Response: Thank you for noticing. The correct format is “and 040802 (Saginaw)”, which was revised in the manuscript.

Comment: P3426 “constitute the remaining 16.2 percent of land cover” ... but these percentages do not add up to 100%, so “remaining” can’t be the right word.

Response: Thank you for noticing the typo. The correct percentage is 29.5 as presented in table 1. In addition, it was revised in the manuscript.

Comment: In general, the paper is written carelessly. Why is “yr” used as an abbreviation for “years”?

Response: The original manuscript submitted to the journal does not contain any abbreviation for year. The editorial support of the journal converted all “year” and “years” to “yr”. Therefore, any concern should be referred to the journal’s editorial support.

Comment: P3426, Lines 4-9. Provide a reference for this data.

Response: Thank you for your comment. The references were added to the start of the sentence and reads as “Based on pre-settlement land use data obtained from Michigan Natural Features Inventory, Original Vegetation Cover of Wisconsin, and Land Cover of Illinois for the early 1800’s, the area of interest has gone though a significant land use changes in the past 200 years.” in section 2.1.

Comment: P3426, 11. “that is well-suited for studying the large scale impacts of land use changes” This is very debatable. At least, SWAT suffers from the same general limitations of any distributed hydrological model, in having very large uncertainty in



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model structure, model inputs, initial conditions, and parameter values. The authors need to provide more significant justification for their choice of SWAT and be more critical about the limitations of their study arising from the large model uncertainties.

Response: Thank you for your comment. Referring to the SWAT model website, this sentence was highlighted in the home page of the model, "SWAT is a river basin scale model developed to quantify the impact of land management practices in large, complex watersheds." SWAT is a physically based model that has gained international acceptance as a robust watershed model as evidence by nine international conferences dedicated solely to the SWAT model. In addition, more than 660 peer reviewed SWAT related papers have been published (https://www.card.iastate.edu/swat_articles/citations_by_model.aspx) in which more than 70 articles related to hydrologic assessment (https://www.card.iastate.edu/swat_articles/index.aspx). In fact, SWAT is the most reviewed watershed model in the world. The following sentences were added to the manuscript to address your comment in section 2.2. "SWAT has gained international acceptance as a robust watershed model as evidence by hundreds of peer-reviewed and conference publications. In addition, the model has been widely used by federal and state agencies (Gassman et al., 2007)."

Reviewer did not specify in comparing to which model SWAT "having very large uncertainty in model structure, model inputs, initial conditions, and parameter values". However, authors agree that there is a level of uncertainty present in any scientific work including modeling, field studies, monitoring, etc. Evaluating different aspects of uncertainty (model structure, inputs, and parameters) is beyond the scope of the study and can be discussed in a separate paper. In addition, output parameter uncertainty analysis cannot be performed for many hydrological parameters under current scenario and no parameter for pre-settlement scenarios because the true values are unknown. Additionally, as it was discussed by Yang et al. (2008) application of uncertainty analysis for complex hydrological models such as SWAT are computationally too expensive



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for straightforward use. However, we agree with the reviewer about the important of uncertainty analysis. As a matter of fact, the first author of this manuscript has published a paper in 2003 discussing the uncertainty within the hydrologic and water quality models specifically SWAT (Sohrabi et al., 2003). Therefore, the following sentence was added to the manuscript to acknowledge the importance of model uncertainty in section 2.5 "In addition to calibration, uncertainty analysis is important for distributed watershed models such as SWAT. Sources of structural uncertainty for these types of models include unaccounted processes within the model and over-simplification of model processes. However, performing uncertainty analysis is computationally expensive and time consuming for complex hydrological models, and therefore is not within the scope of this research (Yang et al., 2008)."

We also added the following sentence to the "Conclusion" section "However, due to the important role of uncertainty analysis in the decision making process for water resources, it is recommended that future studies be performed to evaluate different sources of uncertainty to increase confidence in the model results."

Reference: Yang, J., Reichert, P., Abbaspour, K.C., Xia, J., Yang, H. Comparing uncertainty analysis techniques for a SWAT application to the Chaohe Basin in China. J HYDROL 358, 1-23, 2008.

Sohrabi, T.M., Shirmohammadi, A., Chu, T.W., Montas, H., Nejadhashemi, A.P. Uncertainty analysis of hydrologic and water quality predictions for a small watershed using SWAT2000. Environmental Forensics, 4:229–238, 2003

Comment: P3426, 22. "A daily water budget in each HRU is calculated based on daily precipitation, runoff, evapotranspiration, percolation, and return flow from subsurface and groundwater flow" I'm not sure what this means: a budget in the HRU means balancing the inputs, outputs and storage from an HRU, but the rest of the sentence includes internal fluxes and does not include storage.

Response: Thank you for your comment. The sentence was re-written in section 2.2 to



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address the concern. “Based on daily precipitation, runoff, evapotranspiration, percolation, subsurface return flow, groundwater flow, and changes in water storage, a daily water budget in each HRU is calculated”

Comment: P3427. “The SCS curve number method estimates surface runoff from daily rainfall using initial abstractions (surface storage, interception, and infiltration prior to runoff) and a retention parameter (varies based on changes in soil, land use, management, and slope as well as temporarily due to changes in soil water content)” It will be interesting to see how the authors estimate all the parameters of these components for all relevant land uses, and how they handle the uncertainty. Same applies to all the model components.

Response: Thank you for the comment. The parameters that the reviewer has referred to are already defined in the SWAT model based on extensive literature review specially from the Natural Resources Conservation Service (NRCS) National Engineering Handbook (NEH) http://www.mi.nrcs.usda.gov/technical/engineering/neh.html, physiographic and climatological characteristics. For the uncertainty comment please see the response to the previous comment (2 above).

Comment: P3427,14. “Daily PET values obtained from monitoring can also be incorporated into the model” So, was this the method used? It’s not clear which method was adopted

Response: The following sentence was added to section 2.2 to address the comment. “However, since observed PET values were not available, daily PET values were estimated using the Penman-Monteith method.”

Comment: P3427, 15. What is “total PET”?

Response: Thank you for your comment. The word “total” was removed from the manuscript.

Comment: P3427, 22. This is an incomplete description of soil moisture modelling: e.g.



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how is vertical movement of water calculated, what is assumed about the distribution of evaporation losses over depth, what is the lower and upper boundary condition?

Response: The soil moisture modeling was described in detail in SWAT theoretical documentation (Neitsch et al., 2005) http://swatmodel.tamu.edu/media/1292/SWAT2005theory.pdf in chapter 2:3 pages 141 – 161, which is referenced in the manuscript. Due to the page limit, it is not possible to have a complete description of every assumption that the model makes. Readers are encouraged to obtain more specific description from the SWAT manual.

Comment: P3427, 25. The English is generally good, but there are lots of mistakes, for example here "the" is missed out from in front of "water budget" There are lots of other small errors which I have not listed here. With better quality of writing, the paper would be shorter and clearer.

Response: Thank you for your comment. Authors have attempted to correct any small errors within manuscript, including the one mentioned above.

Comment: P3428, 15. Why is this under the "Groundwater" subheading?

Response: The subheadings describe the start of separate sections that were explained in greater detail. This includes "Soil Water Relationship", "Groundwater", etc.

Comment: P3429, 8. "based on the survey performed in mid-1800" Ambiguous: in what year(s) was the survey performed?

Response: The data was collected during 1832 to 1866, as described R.W. Finley in 1951 and added to the manuscript. The title is the Original Vegetation Cover of Wisconsin, Ph.D. Thesis, University of Wisconsin, 1951.

Comment: P3429, 20. "boundary, slope, etc" I'm interested in what the 'etc'

Response: additional characteristics including river channel elevation, subbasin area, average watershed elevation, and flow path were added to the end of the sentence in



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section 2.3.

Comment: P3429. includes: please list all the indices used.

Response: All indices were listed in the Appendix section.

Comment: P3429, 23. "These differences may have significant impacts on watershed hydrologic responses such as stream flow and evaporation in two regions" Please briefly state why – different vegetation and/or climate?

Response: "These" refers to elevation not vegetation and/or climate

Comment: P3430, 15. & 16. "varies from 674mm to 1115mm" Varies within the region; or over the years of the record?

Response: Thank you for the comment. Our intention is within the region; therefore, "spatially varies" was added to the sentences to further clarify any confusion (section 2.3).

Comment: P3431, 4. Tables 3a and 3b are not self-explanatory. How were these ranking arrived at?

Response: Thank you for your comment. The following sentence was added to section 2.4, "In these tables, the overall rankings for each watershed parameter were calculated based off the median and mean of individual rankings for all watersheds."

Comment: A short description of all the parameters is needed.

Response: All parameters were listed in the "Appendix" section.

Comment: Is it necessary to include Tables 3a and 3b, or could the main results just be summarised in the text?

Response: Authors prefer to keep the tables since removing the tables might raise more questions about how the individual and overall rankings were performed.

Comment: P3431, 20-25. Then was the sensitivity analysis useful? If it is not mea-



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suring the sensitivity that is actually important when it comes to calibrating the model, then could it have been designed better? Or were all the parameters sensitive, in which case presenting the ranks is not especially helpful in this context.

Response: In general, the sensitivity analysis was useful. However, as it was discussed in section 3.1, attention should be taken to determine the true importance of sensitive parameters by considering their placement in model algorithms, since the most sensitive parameters may not always be appropriate for use in model calibration. Therefore, it is appropriate to recommend better design of the sensitivity analysis within the SWAT model. The following sentence was added to the end of paragraph in section 3.1. "It is recommended that in the future versions of SWAT, sensitivity analysis is redesigned to avoid this type of problem".

Regarding your question about "were all parameters sensitive..."Not all parameters are sensitive, however among the ones that are, some parameters are more important than the others, therefore ranking is useful and/or necessary for calibration.

Comment: P3431, 22. "Parameters that were not identified as sensitive but used in calibration were applied to match the model with naturally occurring processes in the watershed" Not clear to me what this means.

Response: The SWAT sensitivity analysis toolbox only allows the selection of 42 parameters for sensitivity analysis. However, SWAT model contains thousands of parameters, which were not included in the toolbox. Therefore, the modeler should incorporate the best understanding of natural processes to calibrate the model in a way that reflects more natural and realistic processes within a specific watershed.

Comment: P3432. "However, by setting up the model for pre-settlement scenario based on current climatological variables (e.g. precipitation temperature, etc. for the period of 1990–2008) we can accurately compare the results of land use changes in the region while eliminating the climatological difference" But this avoids the main question: how do you estimate the parameters for the pre-settlement conditions if you have no



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calibration data? To side-step this issue, and then to say that the comparison is done "accurately" is very unconvincing. This is addressed in the following sentences by stating that the calibrated parameters were applied to the pre-settlement conditions: this is confusing unless a more detailed explanation of what assumptions were made about mapping parameters/HRUs from modern to pre-settlement conditions. "However, the underlying assumption is that models such as SWAT were developed to evaluate hydrologic and water quality impacts of landuse change without limitation regarding the type, amount, and nature of landuse change" This is not the only underlying assumption: some assumption has been made about the applicability of calibrated parameters to historic conditions.

Response: Thank you for your comment. Models are built based on our understanding of natural system and models are widely used to predict future climate, landuse, etc. scenarios where suitable field data are not available. As it was described above "the underlying assumption is that models such as SWAT were developed to evaluate hydrologic and water quality impacts of landuse change without limitation regarding the type, amount, and nature of landuse change". With agreement on this assumption, the calibrated parameters are applicable to any landuse change scenario within the watershed. This is common practice for all modeling studies, otherwise models cannot be used for scenario outside of the current calibrated conditions.

Regarding your concern about "what assumptions were made about mapping parameters/HRUs from modern to pre-settlement conditions.", An assumption for within the SWAT model is that areas with similar landuse, soil type, and slope are assigned to the same HRUs. This assumption was not changed with pre-settlement landuse. Therefore, it was not stated in the document as a new assumption.

Comment: Also, I'm not clear what is meant by this sentence. Is it basically assuming that SWAT is accurate however parameters are adjusted for land use change? Was the calibration done manually, or automatically?



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Response: The whole notation of calibration is to improve accuracy of the model. No tool is accurate without adjustment or calibration. Models are not an exception. The accurate SWAT that you are referring to is the calibrated model.

Generally, it is advised to start the calibration process with manual calibration and then with autocalibration to accelerate the calibration process (Srinivasan, 2006). This is the practice that we adopted here. What is assured a reasonable model output is the selection of appropriate model parameters that are sensitive and have been matched to the model with naturally occurring processes (has physical meaning). The first step in the model calibration, whether manual or autocalibration is to select a series of parameters, which have the above quality. In manual calibration, the user changes parameters one at the time (within a tolerable range) until the result of the model (e.g. streamflow) is acceptable based on the objective function. In the autocalibration, a series of parameters were selected based on the above criteria, like the manual calibration, then a allowable range was defined for each parameter, and finally, the model was tested for the parameters until the predefined objective function is reached. Therefore, from conceptual point of view, there is no difference between manual and autocalibration.

Reference:

Srinivasan, R., 2006, Advance SWAT Training, Texas A&M University, College Station, TX.

Comment: P3433, 2-5. This is introductory material and could be deleted from here.

Response: Thank you for your comment. The following sentences were deleted “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: P3433, 14. median and mean of what? Why were both used – what differ-



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ent relevant information are they expected to give? Needs explained in the methods section.

Response: Thank you for your comment. The sentence was rewritten to explain that mean and median are corresponding to rankings of watershed parameters. Please see section 3.1. Read as "Two criteria (mean ranking and median ranking for each watershed parameter) were selected to identify the most influential parameters, which affect daily flow rates." The following sentence was also added earlier in section 2.4, "In these tables, the overall rankings for each watershed parameter were calculated based off the median and mean of individual rankings for all watersheds." In response to selection of both median and mean, the following was added to the manuscript to justify the use of the two above criteria (section 3.1). "In the case that mean of two watershed parameters' rankings are the same, the median value was used in determining the overall ranking."

Comment: In Table 3, what do W1 and M1 mean? In general, I think sensitivity analysis of SWAT model parameters is a good idea. However it is not clear enough how this section is contributing to the aims of the paper. The authors should make it clearer (in the methods section) how sensitivity analysis can be used to evaluate land use impacts.

Response: Thank you for the comment. WI is abbreviation for Wisconsin and MI is the abbreviation for Michigan. Footnotes were added to both table 3a and 3b. The sensitivity analysis shows which parameters in the model are more sensitive to land use change.

The aim of this paper is to examine the effects of land use change on hydrologic fluxes at both local and regional scales as well as understanding the underlining mechanisms. Performing sensitivity analysis and identifying sensitive parameters can help us to better explain why and how the model algorithm responds to land use change and why some parameters become more sensitive than the others under certain land use sce-



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narios. The following sentence was added to the method section 2.4 to address the concern “In addition, performing sensitivity analysis and identifying sensitive parameters can help us to better explain why and how the model algorithm responds to land use change and why some parameters become more sensitive than the others under certain land use scenarios.”

Comment: P3434, 1-7. I found this confusing

Response: As it was described in the manuscript since the SWAT model does not assign different Rchrg_Dp values to different land uses, change in ranking of the Rchrg_Dp only caused by the Cn2 parameter resulting in more recharge and less runoff.

Comment: P3436. This is all description of method, so should be in the previous section, not in the results section.

Response: Thank you for your comments. These descriptions were moved to the method section “2.5 Model Calibration and Validation” .

Comment: P3436, 1-11. The description and use of R2 could be deleted, because R2 will not contribute anything which ENS does not. The same applies to the RMSE.

Response: Thank you for your comments. The R2 and RMSE were removed from both the manuscript and table 4.

Comment: If the authors wish to use multiple objectives, then it would be better to use three objective functions which are significantly different (e.g. high, medium and low flow functions)

Response: Please see the above response.

Comment: P3436, 18-20. “the impacts of low values in time series (e.g. baseflow or lateral flow) are neglected”. This is incorrect: all values are in the time series are included in the ENS.



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Response: Thank you for your comments. The sentence was revised in section 2.4. “Since the difference between observed and model results is squared in this method, the impacts of low values in time series (e.g. baseflow or lateral flow) have little impact to overall ENS.”

Comment: In addition, Nash-Sutcliffe coefficient of efficiency is not sensitive to over or under predictions for low flow scenarios (Krause et al., 2005)” This is too general a statement. In cases, the ENS is very sensitive to low flow performance: it depends on the relative errors in and relative amount of low flow data compared to high flow data. E.g. in arid regions, where 99% of the data may be ‘low’ flows, the ENS is likely to be more sensitive to the low flow than the high flow. “is often not sensitive” would be better.

Response: Thank you for your comment. The sentence was revised in section 2.4. “In addition, Nash-Sutcliffe coefficient of efficiency is often not sensitive to over- or under-predictions for low flow scenarios (Krause et al., 2005).”

Comment: P3437, 17-25. This is not convincing. There are very few papers which treat ENS=0.2 as acceptable. I find it hard to believe that Di Luzio and Arnold interpreted 0.15 as satisfactory without big reservations. If the authors have to lower the acceptable ENS threshold to such a low value then a more useful and interesting paper would be about why SWAT worked so poorly in this region: with such low performance it’s difficult to put much faith in the calibrated parameter values and hence in the conclusions about land use effects. Looking at Table 4, however, the discussion of why the model performs so poorly, would only be for one gauge: this really needs to be done.

Response: Thank you for your comments. The authors agree that the ENS criteria should be higher, therefore, the acceptable criterion for daily basis evaluation was increased to 0.4. The following paragraph was revised in section 2.5. “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



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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: P3438, 1-15. This is a description of method and should be in the earlier section.

Response: Thank you for your comment. The paragraph was moved to the methods section.

Comment: P3438, 21-28. The correlation analysis seems dubious because it is using model results as the data, which are not independent of each other (presumably all the simulations of a particular land use change are generated using the same pair of parameter sets; so we know there is a consistent change in response; and the same parameter error is being applied to generate each data point). In short, any statistical significance test seems flawed as there is no stochastic component to the data.

Response: In general, statistical analysis performed on data values obtained from models is typically criticized because the "increase" or "decrease" observed in all observations is driven by the model parameters not for the variables of interest. For instance, if your model has a parameter that increases runoff "evenly" in all observations, then correlation coefficient in the response values will be high because they receive the same influence from the hydrological model. However, this is not the case in this study because:

1. Hydrological values were obtained in pre-settlement with a particular configuration of land use. Later hydrological values were obtained in current land use with a new



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configuration of land use (The new configuration of land use in each subbasin has nothing to do with the hydrological model). Therefore, adding a stochastic component to the data that has nothing to do with the hydrological model used to derived values.

2. Although hydrological model parameters were kept constant, the model could respond differently to a different configuration of land use (i.e. current vs. pre-settlement), thus affecting differentially to the response variables (stochastic process). In other words, errors are not constant (or equal) across all observations.

3. Correlation analysis was performed between "change" in hydrological values and percent of land use conversion (a variable that has nothing to do with the hydrological model). Therefore, it can be argued that the percent of area of conversion is an external variable that is not influenced by the hydrological model. The main point is that we are not only using model data in our correlations.

Comment: P3439, 8-15. Could be deleted: not important for the reader to know details of this.

Response: Thank you for your comments. The following sentences were deleted "Normality was assessed using normal probability plots and the Kolmogorov-Smirnov test. The null hypothesis of normal distribution was rejected in all the studied variables. Most of the hydrological variables expressed as percent showed skewed distributions deviating from normality (e.g. percent changes in water content, percolation, surface runoff, lateral flow), some variables showed strong evidence of outliers (e.g. percent changes in water content, percolation and surface runoff). Percent of land use conversion was not normally distributed as indicated by the Kolmogorov-Smirnov test ($D = 0.26$, $p < 0.01$).".

Comment: P3439, 16-25. Much of this detail could also be omitted. Again, much of this is describing method, so why not put in Section 2?

Response: Thank you for your comment. The decision to use nonparametric measures



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was made after observing the results which showed that the data was not normally distributed. Because this analysis was based off of results, authors prefer to discuss these methods in the current section of the manuscript.

Comment: P3441, 21. “surface runoff, lateral flow” does this mean “lateral subsurface flow” otherwise what distinguishes it from surface runoff? There are many similar examples throughout the paper, which I have not listed, where careless writing makes it confusing.

Response: The term “lateral flow” was adopted from the SWAT manual to describe the lateral subsurface flow. However, in order to address your concern, the term “lateral subsurface flow” was used throughout the manuscript.

Comment: “baseflow minus transmission loss”: this also confused me: why are transmission losses linked with baseflow; and why are transmission losses considered a loss in yield?

Response: The transmission loss was not linked to the baseflow. It is a separate component when calculating the water yield, which accounts for leaching through the streambed.

Comment: P3443, 1. Does this mean that modelled yield reduces under deforestation? If so, this seems contrary to most of the literature, and if correct is probably worth discussing.

Response: Thank you for your comments. Most hydrological behaviors are complex and site specific. Therefore, it is very difficult, if it is not impossible, to expand the results of one study to define universal hydrological responses to specific stimuli (e.g. landuse change or climate change). Hundecha and Ba′rdossy (2004) presented a number of opposing findings concerning land use change impact assessments, while Hibbert (1967) showed significant relationships between deforestation and increased in water yield, and Langford (1976) study showed no relationship. Even in this study,



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we observe opposing results for hydrological fluxes in different watersheds (Table 6 in the revised manuscript or table 7 in the original maniscript).

Reference: Hundecha, Y., Ba'rdossy, A., 2004. Modeling of the effect of land use changes on the runoff generation of a river basin through parameter regionalization of a watershed model. Journal of Hydrology 292, 281–295.

Sampaio, G., Nobre, C., Heil Costa, M., Satyamurty, P., Soares-Filho, B.S., Cardoso, M. 2007. Regional climate change over eastern Amazonia caused by pasture and soybean cropland expansion, Geogyscal Research Letter 34: L17709.

Zhang, Y.K., Schilling K.E., 2006. Increasing streamflow and baseflow in the Missis-sippi River since 1940s: effect of land use change, Journal of Hydrology. 324 (1-4), 412–422.

Meanwhile the following paragraph was added in section 3.5 to address your concern. "A closer look at model parameters influencing water yield shows that the leaf area index for the forested lands never reached its maximum value because the optimal temperature for plant growth was not consistently reached during the course of the growing season and period of study. Therefore, lower rates of evapotranspiration were observed in forestlands than agricultural land. This could contribute to the reduction in water yield under deforestation. However, most hydrological behaviors are complex and site specific and for this study the conversion of wetlands, rangeland and forested areas at different level may cause decreases in the water yield."

Comment: Figures 7, 8 and 9 not all needed: be selective in which you show.

Response: Thank you for your comment. However, authors believe that presenting these figures provide valuable information about the spatial variability of the different hydrological components that were included in the study.

Comment: Either Table 5 or Table 6: I don't think including both is necessary.

Response: Thank you for your comment. Table 6 was removed.



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Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/8/C1672/2011/hessd-8-C1672-2011-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 3421, 2011.