

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 #1

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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. 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

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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.

P3423, Line 21. Is ET defined somewhere? 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 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 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. 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. 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. 26. “040802 and Saginaw.” What does this mean? Is 040802 the same as Saginaw?

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. In general, the paper is written carelessly. Why is “yr” used as an abbreviation for “years”? Lines 4-9. Provide a reference for this data. 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 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.

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”

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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.

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. 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 15. What is "total PET"? 22. This is an incomplete description of soil moisture modelling: e.g. 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? 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. P3428, 15. Why is this under the "Groundwater" subheading? P3429. 8. "based on the survey performed in mid-1800" Ambiguous: in what year(s) was the survey performed? 20. "boundary, slope, etc" I'm interested in what the 'etc' includes: please list all the indices used. 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? P3430. 15 & 16. "varies from 674mm to 1115mm" Varies within the region; or over the years of the record? P3431, 4. Tables 3a and 3b are not self-explanatory. How were these ranking arrived at? A short description of all the parameters is needed. Is it necessary to include Tables 3a and 3b, or could the main results just be summarised in the text? 20-25. Then was the sensitivity analysis useful? If it is not measuring the sensitivity that is actually important when it comes to calibrating the model, then could it have

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been designed better? Or were all the parameters sensitive, in which case presenting the ranks is not especially helpful in this context. 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.

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 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. 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? P3433, 2-5. This is introductory material and could be deleted from here. 14. median and mean of what? Why were both used – what different relevant information are they expected to give? Needs explained in the methods section. 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. P3434, 1-7. I found this confusing

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P3436. This is all description of method, so should be in the previous section, not in the results section. 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. 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) 18-20. "the impacts of low values in time series (e.g. baseflow or lateral flow) are neglected". This is incorrect: all values in the time series are included in the ENS. "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.

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. P3438, 1-15. This is a description of method and should be in the earlier section. 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. P3439. 8-15. Could be deleted: not

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important for the reader to know details of this. 16-25. Much of this detail could also be omitted. Again, much of this is describing method, so why not put in Section 2? 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. "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? 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.

Figures 7, 8 and 9 not all needed: be selective in which you show. Either Table 5 or Table 6: I don't think including both is necessary.

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