

Interactive comment on “The importance of parameterization when simulating the hydrologic response of vegetative land-use change” by Jeremy White et al.

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Received and published: 28 March 2017

This paper deals with uncertainty in hydrologic model results, with a focus on land-use change. Although the results do not really surprise me - 'calibration' on discharge time series, evaluation on other variables related to ET is expected to be difficult, maybe especially with a conceptual model like SWAT -, it certainly is of added value to the recently growing literature that demonstrates the enormous uncertainty in hydrologic models that is often ignored or underestimated in current day modeling practice (e.g., indeed, by selecting only a sample of parameters). I think the paper is well written (besides typo's and/or spelling errors which have been referred to by reviewer 1) and well structured, with an important message. I would, however, like to give some food

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for thought which might improve the interpretation of the results;

- Daily discharge observations are used for a catchment of 1.4 km², I guess the response time of the catchment is much shorter than this daily time step. In this way, probably some essential hydrological processes cannot be captured in the 'calibration'-procedure. How do you think this affects your results?

- Figure 2 gives the impression that the catchment shows intermittent streamflow behavior; it seems that there are long periods of no-flow. Calibration on discharge in no-flow periods is a tricky task; Do you expect that a longer calibration-period could improve the results? Of what would happen if you repeat the whole exercise in a wetter catchment?

Like I said before, I think it is an interesting study with interesting results that is probably representative for many modeling studies in which the uncertainty is underestimated. I do think, however, that maybe a more thoughtful calibration could potentially improve the results (I am not sure, of course; calibration is not a panacea. Furthermore, the calibration-procedure applied in this study is probably representative for current modeling practice). I would be interested to see this in the discussion of the paper.

Concerning the sensitivity analysis (p.5, l.28-29); I agree with the authors that selecting model parameters for calibration is often subjective. However, I think the common path in modeling is to conduct a sensitivity analysis (which is the subjective part, because; global or local method? which parameters to include? what parameter boundaries?), and based on that identify the parameters for calibration, whereas the authors chose a different approach; first select the parameters, and after that conduct a sensitivity analysis. Could you explain why you chose this procedure? Furthermore, for the readability, I would suggest to move section 2.7 to earlier in the methods, especially because you start with the sensitivity analysis in the results.

Last point; You have ET data at your disposal. This provides a great opportunity to use ET for your calibration. I would be really interested to see how the selection of

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behavioral parameter sets would be influenced if you add an ET-criterion, and how this would affect the QOIs related to ET. This does not require any additional calculations and potentially you could make a strong case to increase ET observations in order to improve the modeling of land-use change impacts (in other words; you could provide constructive suggestions to decrease the uncertainty. Or not, dependent on the results). Maybe this extra exercise is not really necessary in order to provide sufficient body for a paper, but it certainly could provide a strong message.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2017-111, 2017.