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Hydrology and Earth System Sciences Discussions

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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Consolidated Replies to Online Comments

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1 Introduction

In this document, we respond to each comment raised by the reviewers. The revised manuscript is attached.

2 Reply to RC1 - John Doherty

We appreciate Dr. Doherty's encouraging review. We have made substantial revisions to improve the grammar the revised submission. Regarding the choice of subjective likelihood function, it is possible that a more QOI-focused likelihood function could be found and applied to yield a greater decrease in QOI-5. The 3-component likelihood

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function was selected because it is used widely within the hydrologic modeling community and we were interested in assessing "common practice" in the simulation of brush management.

3 Reply to RC2 - Patrick Belmont

We appreciate Dr. Belmont's review and we agree that parameterization is an often overlooked but critical aspect of model usage.

1. *P3 Line 14: I'm okay with the authors mostly referring readers to the 2011 paper for information about the study area. However, it would be helpful to include at least mean annual precipitation and temperature. A brief explanation of the seasonal pattern of rainfall would also be helpful. Readers should not have to look up another paper for this basic information.* We added a brief description of the average annual rainfall to the manuscript.

2. *P4 Line 12: The technique used to spatially average the precipitation data should be specified.* The precipitation data were combined via arithmetic averaging to yield a complete (filled), 5-minute precipitation record for the model. We added this information to the manuscript

3. *P4 Line 18: Did you evaluate how well the NCEP data correspond to your instrumental measurements for time periods during which your instruments were functioning properly? Documenting the error for days on which rainfall occurred would be useful.* We agree that the NCEP data may in fact be of lower quality and accuracy compared to the site-specific precipitation data. We did not specifically evaluate the error in the NCEP data, however, we did treat precipitation inputs as uncertain in the analysis, which should account for error in the NCEP

precipitation estimates, among other errors. We have added this information to the manuscript. $\int$

4. *P6 Line 30: The authors could provide more explanation of the advantages and disadvantages of these two types of parameterization.* The only advantage of using the reduced parameterization is the improved computational demand required to implement an automated calibration. However the disadvantage of the reduced are numerous, including under-estimation of uncertainty in quantities of interest (as we show). The full parameterization requires more sophisticated and programmatic approaches to calibration, but includes that added benefit of an improved ability to express model input uncertainty. We have added similar language to the text.

5. *P 8 Line 30: Are these midpoint values the same as the default values for SWAT2012? If so, that's fine: : :it's what most modelers would do, but the authors may want to clarify this point. If not, some justification is needed for using these values rather than the default values.* The midpoint of the basin-scale parameters excluded from the reduced parameterization does not necessarily correspond exactly to the values yielded by ArcSWAT. However, these midpoint values are still within the range of "acceptable" as defined by literature sources and site-specific expert knowledge. Additionally, for all HRU and precipitation multipliers (the vast majority of parameters in the full parameterization), the midpoint is 1.0, which essentially removes there affects from the analysis. We have added similar language to the text.

6. *P 9 Line 9: Each of these measures quantify slightly different components of model performance. The authors might want to include 1-2 sentences to explain the differences between the three and advantages of using all three.* We have added a brief description of the utility of NSE, percent bias and coefficient of determination as an objective function and how using these three measures together increases their effectiveness at identifying realizations the reproduce several aspects of the conditioning period observed streamflow.

7. *P 10 Line 6: This is still a very large number of realizations. It would be useful to know how many of them are effectively duplicates of one another. Also, it could be helpful to modify the conditioning measures to select for a narrower range of runs.* Each of these realizations were drawn stochastically from the Prior distribution—Figure 3 in the manuscript shows how these realizations fit the three conditioning measures. The number of behavioral realizations is function of the conditioning measures and the size of the prior ensemble. That is, we could reduce the size of the behavioral ensemble by simply reducing the size of the prior ensemble. Furthermore, we feel the thresholds we selected for the three conditioning measures are appropriate and also commensurate with current hydrologic modeling practice. We note that requiring realizations to pass very strict conditioning measures risks overfitting with respect to the QOIs.

8. *P 11 Line 14: I agree with the authors that the possibility of a net increase is not entirely unexpected. Recognizing that the cutoff thresholds for the evaluation measures were somewhat arbitrary (if in line with most other literature) it would be interesting to know if the realizations that indicate an increase in ET are eliminated if stricter evaluation measures are applied.* We agree with Dr. Belmont that an increase in ET following brush management is not entirely unexpected and that stronger conditioning (through application of more strict conditioning measure thresholds) may affect the behavioral distribution of QOI-5. However, as shown on figure 8, conditioning of the full parameterization model has shifted the distribution slightly *towards* the positive ET region, although, in general, the behavioral distribution is only slightly affected by conditioning. Therefore, we would speculate that "tighter" conditioning measures would not eliminate the possibility of a simulated net increase in ET following brush management.

4 Reply to RC3 - Lieke Melsen

We appreciate the review by Dr. Melsen, especially the remarks regarding the value of the ET data.

1. *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?* We also recognize that our model is operating a lower temporal frequency than the actual watershed. Indeed, all models of natural systems are simplifications and must operate at lower spatial and temporal frequencies than the natural systems they simulate. However, we would speculate that this form of model simplification is not adversely affecting our results for the following reasons:

    (a) the focus of the modeling analysis is long-term water budget components
    (b) both parameterizations reproduce observed streamflow acceptably well
    (c) both parameterizations reproduce conditioning and forecast period verification QOIs

    It is possible that higher-resolution conditioning data might condition additional parameters compared to the daily streamflow data. However, this conditioning is likely limited to parameters that influence high-frequency runoff generation processes, not necessarily parameters that influence long-term water budget components.

2. *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 panace. 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. We agree with Dr. Melsen that a more "thoughtful" objective function could possibly yield a narrow behavioral distribution for QOI-5. However, we specifically select the three conditioning measures formulated from daily streamflow observations based on their wide-spread use on the hydrologic modeling community. We have added some discussion to this affect to the manuscript.

3. *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.* We appreciate this comment and have added to the manuscript to clarify this process and have reordered the sections of the manuscript. In short, we chose to use GSA to investigate which (uncertain) model inputs influence the conditioning measures (the calibration), the QOIs (the purpose of the model) or both. By including most (if not all) uncertain model inputs in the GSA and investigating both the conditioning and model purpose (e.g., QOIs) with GSA, practitioners can gain a clearer understanding of which model inputs are important for reproducing the past as well as which model inputs are important to simulate the QOIs.

4. *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 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 it not really necessary in order to provide sufficient body for a paper, but it certainly could provide a strong message.* We agree with Dr Melsen that the conditioning period ET may provide valuable conditioning of the parameters that influence QOI-5. We plan to address the value of the ET data for conditioning in another manuscript focused on dataworth analyses for this modeling analysis. However, we have added an additional paragraph to the discussion that also address the importance and potential value of the ET data.

5 Reply to RC5 - Tammo Steenhuis

We appreciate Dr Steenhuis' review. While Dr Steenhuis indicated the manuscript was "poorly written", we note that the other reviewers did not have issue with the construction or organization of the manuscript beyond some minor grammatical mistakes. We also note the model was conditioned with streamflow and validated with ET and streamflow under changed land-use conditions.

1. *Streamflow is simulated using the Green and Ampt approach that is likely marginally sensitive to differences in amount of water evaporated by the plants either with trees or without trees. The variation in conductivity due to crust formation is likely a much more sensitive parameter The other words overland flow*

cannot be used for estimating evaporation. Baseflow could be used, but it is not clear from the article if any baseflow separation was done. Moreover, overland flow once generated during the most intense part of the storm might infiltrate down the hill (Stomph et al 2012) that is not simulated by SWAT while it may greatly affect the amount of surface runoff. Finally, the rainfall could be highly variable over the watershed affecting the runoff greatly with the Green and Ampt approach. The authors took the average precipitation of four stations. At a minimum it should have been investigated if using the four precipitation measurements could have better described the streamflow that the brush management. While Dr. Steenhuis points to several potential structural problems with SWAT as well as other potential conceptualizations of the system. Agreed, no model is perfect and SWAT has limitations that have been well documented in the literature. Indeed, one of the focuses of our study was to quantify the uncertainty using common, industry-standard tools/approaches so that our results have a wider applicability., Nonetheless, several thousand realizations from both the reduced and full parameterization models that fit the conditioning-period streamflow exceptionally well, according to commonly-accepted metrics. Furthermore, despite these shortcomings, the behavioral distributions from both parameterizations reproduce the two verification QOIs well.

2. *The authors write "Note that many of the most influential parameters, specifically precipitation multipliers, plant growth parameters, and HRU scale parameters, are not in the reduced parameterization and are not included in typical hydrologic modeling analyses (Arnold et al., 2012b)" Because other not experienced users do it wrong that is not a good reason not too include the parameters describing the system. Of course, under these circumstances the model fails with this reduced parameter set. Using this set of parameters does not advance science as is expected from a published manuscript*. Firstly, we do not feel that referring to all of the cited works in Arnold (2012) as "not experienced" is a fair or construc-

tive comment. To our knowledge, all of the works cited in Arnold (2012) were subjected to peer review and are of high quality. As stated in the manuscript, we selected the reduced parameterization based on standard, current modeling practice. We then show that, indeed, the reduced parameterization is able to fit the observed conditioning-period streamflow well according to common metrics. We feel this is a validation of current modeling practice in as much as the reduced parameterization can reproduce the past. Our point is that just because the reduced parameterization reproduces the past streamflow doesn't indicate the reduced parameterization model is acceptable for robust simulation of the QOIs.

3. *The authors never question a priori the suitability of the SWAT model whether there is a chance that the model could simulate differences in evaporation based on the streamflow record before going through all the calculations and essentially proving that the SWAT model was not suitable for this problem. Would the authors have chosen an appropriate model that can simulate plant and root development together with evaporation, the results could be completely different and likely much more accurate. The article is all about parameters uncertainty while model uncertainty should have been investigated as well at a minimum.* As previously noted, both parameterization are able to fit the conditioning-period acceptably well according to commonly-accepted metrics. Furthermore, the SWAT model has emerged recently as a popular tool for simulating many hydrologic processes (beyond brush management and land-use change) For example, see https://www.card.iastate.edu/swat_articles/citations-list/