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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Hydrological hysteresis in catchments and its value for assessing process consistency in conceptual models" *by* O. Fovet et al.

Anonymous Referee #3

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HYDROLOGY AND EARTH SYSTEM SCIENCE DISCUSSIONS

Review of Manuscript # hessd-11-5663-2014

Hydrological hysteresis in catchments and its value for assessing process consistency in conceptual models

Authors: Fovet, Ruiz, Hrachowitz, Faucheux, Gascuel-Odoux

1) Overview

This study aims at exploring the value of using hysteretic patterns observed between

streamflow and water storage (both in saturated and unsaturated zones of hillslope and riparian zone) as an additional information for understanding catchment functioning and further as a tool for model diagnostics in addition to the traditional model evaluation metrics derived from streamflow observations. The study area is selected as the Kerrien headwater catchment (10.5 ha) located in south-western French Brittany characterized by a shallow hard-rock aquifer. The data consist of long term streamflow (since 2000) and groundwater levels at three piezometers (since 2001) in addition to the soil moisture at various depths at 2 profiles (since 2010). The authors start with analysis of the observed hysteretic pattern between storage in saturated and unsaturated zones within hill slope and riparian zone and stream discharge. The hysteretic loop pattern has been summarized by an annual hysteresis index quantified by the difference between water storages at the dates of mid-point stream discharge during the recharge and recession periods. After the analysis of observed hysteretic patterns the authors identified four hydrologic periods and emphasized a clearly distinct behavior between riparian and hillslope groundwaters.

Next, ability of hydrologic models in reproducing the hysteretic patterns was assessed in addition to various streamflow signature metrics. Four lumped hydrologic models with increasing complexity were calibrated through Monte-Carlo sampling and pareto surface analysis of four objective functions namely, NSE, NSE-log, flow duration curve based NSE-log, and volumetric efficiency. Analysis of the model results showed that all the tested models were able to produce annual hysteresis loop between discharge and both saturated and unsaturated storage, however representation of the riparian component led to overall improved hysteretic signatures. The authors concluded that addition of internal process observations (storage) in addition to the streamflow observations will likely improve conceptual consistency of models and improve model selection.

2) General Comments:

The manuscript is a relevant and important step towards diagnostic analysis of hydrologic models, with specific contribution through testing internal hydrologic process

representation via comparing observed and simulated hysteretic patterns that exist between storage and streamflow discharge in the selected watershed. I think the topic is of interest to the HESS readership and the manuscript is well written, well-structured and the use of language is generally good. However, part of the analysis is described at an abstract level with frequent citation to a recent unpublished manuscript (Hrachowitz et al., submitted) by the authors. Therefore a clear distinction between the contributions by these two manuscripts should be made in the manuscript. I also suggest below a few cases where explicit discussion of the analysis should be provided. Overall, my assessment for the manuscript is minor revisions. The manuscript could be published after the authors address the comments listed in “Main Comments” and “Minor Comments” sections listed below.

Main Comments:

1)The authors state that 86% of the study area is dominated by agriculture (Section 2.1). A discussion on the source of irrigation water (groundwater?) and how the agricultural use could affect the hysteretic patterns should be provided in the manuscript. Similar discussion related to percentage of snow and its possible impact on the results should be provided.

2)Hysteresis Indices (Section 2.4): The description of the mid-point discharge is not clear. In addition the manuscript lacks a hydrograph, which many hydrologist would very much like to see in a manuscript related to hydrologic processes and models. Therefore the authors should provide a representative hydrograph of the watershed with clear description of the mid-point discharge values on the hydrograph. Below the hydrograph a time series of water levels/moisture levels should be provided again indicating the selected points used in calculating the Hysteresis index. A figure as described above is very important for understanding the HI concept used in the study. My main concern is that the HI concept followed in the manuscript is only specific to the selected watershed. I also think that mid-point discharge could occur multiple times during recharge and recession periods, therefore which time to select should be clearly

described in the manuscript.

3)Model calibration and Evaluation (Section 2.6): It seems that the whole section is taken from Hrachowitz et al. (2014). This should be stated right at the beginning of the section and specific contributions should be clarified. Overall, model calibration and evaluation steps need further explanations and clarifications in the current manuscript. First the selected likelihood measure is not mathematically correct and needs further discussion on the validity and specifically selection of parameter p=10. The authors should include a figure showing only a selected single 2-D representation of the 4-D objective function space to show the projected pareto surface together with the uncertainty intervals. The readers will then be able to understand the calibration and uncertainty analysis procedures with above information. Also, authors state that 13 signatures were used for evaluation, however there is no description of these signatures (perhaps only four is given at an abstract level; Page 5673-Line 22). The signatures used should be explicitly stated.

4)A sensitivity analysis investigating the sensitivity of the hysteretic pattern simulation to the model parameters will significantly improve the manuscript. Currently it is not clear whether the improvements are solely due to the increase in the number of model parameters, or rightly due to addition of new conceptual component to the model as the complexity is increased.

5)Low flow signatures vs. hysteresis patterns: Overall it can be seen that (e.g. Page 5680, Lines 22-23) hysteresis patterns are associated with the low flow signatures as expected. Although authors state briefly, an analysis showing the correlation between the low flow signatures to the hysteretic patterns should be provided. Perhaps a figure could be added showing the low flow signature performance vs. hysteretic index performance of different model structures. Currently low flow signatures are not analyzed independently in the manuscript to investigate their link to the hysteresis signature.

6)Sensitivity of HI to annual rainfall (Section 3.1.5): The annual hysteretic patterns are

sensitive to the timing of rainfall however the sensitivity to the annual rainfall is tested. My concern is that the HI will be sensitive to when the rainfall occurred (recharge period, recession period etc.) but less on the total annual rainfall. A discussion is needed.

7)The authors presented the degree of hysteretic pattern mismatch between models with different complexities (Figures 9 and 10). A modeler will immediately wonder why the authors did not re-calibrate their models to improve the hysteretic patterns? Calibration using hysteretic patterns could provide additional information on the validity of the model structures and help to understand trade-offs in matching flow based vs storage based signatures. This comment is also linked to Comment 6 which is related to Sensitivity Analysis.

8)Conclusion (Section 4): P5686, L4-7: "...They were previously calibrated using classical objective functions and assessed using classical hydrological signatures, and their overall performance at reproducing hysteretic signatures was consistent with their overall performance at reproducing the classical signatures. The analysis of the simulated hysteresis signatures helps to identify why the model may give a right answer for wrong reasons" The above statements by the authors are undermining their work and conflicting. According to the first sentence, one can conclude that the classical hydrologic signatures provided the same information as the hysteretic signatures with respect to the overall performance of the model (both say right model or wrong model). The second sentence is then conflicting since classical signatures are already capable of identifying right model from wrong model (right model for right reason). The authors should provide more in depth discussion about their contribution.

9)The authors should be more careful about the use of plural nouns, e.g. P5684, L23: soils moisture, P5683, L18: parameters sets etc. Please check throughout the manuscript as many more exist.

Minor Comments:

Page 5675, Line 8, Fig.4: An explanation on how the recharge and discharge periods are represented on Figure 4 should be included.

Page 5679, Line 14, Table 4: There is no information related to unsaturated zone in Table 4. Page 5680, Line 24: Figure 3 does not have sub figures a and c.

Page 5682, Line 16: Replace "discharge" with "recharge".

Figure 8a: The markers overlap and hence some of them are hidden behind. Improve the marker representation.

Page 5684, Line 11: Replace "though" with "through".

Page 5684, Line 2: "realisms constraints" choose one.

Page 5684, Lines 6-7: Clarify the sentence.

Page 5683, Line 28: Remove "objectives".

Page 5674, Line 13: Replace "lower that" with "lower than that".

Page 5676, Line 22: Clarify the sentence.

Page 5679, Lines 1-3: Typos with regard to citations.

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