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Hydrology and Earth System Sciences Discussions

Interactive comment on "Top-down analysis of collated streamflow data from heterogeneous catchments leads to underestimation of land cover influence" *by* A. I. J. M. van Dijk et al.

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INTRODUCTION: We thank the reviewer for the considerable effort (s)he has put in reviewing our manuscript and his/her comments, which have helped us improve the manuscript. Below we provide a detailed response to his comments, with reference to the pdf we have attached in our response to the editor, and which contains line numbering and changed text in red font.

COMMENT: 1. Does the process model reconcile the land cover paradox? To answer positively to this question, the authors consider two conditions to be fulfilled : (i) the

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process model needs to perform better than the calibrated Zhang model and (ii) the model needs to provide sensible results for extreme scenarios on land cover modifications. I share the authors' view about the need to fulfil these two conditions but I disagree with the way they deal with the later condition. Indeed, to assess the sensibility of the process model simulations, they compare the outputs of two models, which does not allow validation anyhow. This is clearly not a sufficient condition to answer definitely the posed problem.

RESPONSE: As we now make explicit in the added Caveats section, our goal was not to validate the process model (in any case we are compelled by the argument by the Oreskes et al. (1994) that validation is impossible). (line 197-222)

COMMENT: An interesting alternative test would be to investigate if the performance of the process model is altered when corrupting land cover data, i.e. perform sensitivity analyses. This is almost already done by the authors but the resulting performances of the process model are not shown.

RESPONSE: This suggestion sounds intriguing but we would need some more concrete detail to assess this.

COMMENT: 2. Different inputs lead to different outputs. There is something that deserves more analyses. The process model not only differs to the Zhang model in the way it deals with vegetation cover and resulting estimated evapotranspiration, it also differs in the temporal resolution of the inputs. While the Zhang model only uses pluriannual values of rainfall and potential evapotranspiration, the AWRA-L model uses daily values. I guess that the gap between the performance of the two models is, to a certain extent, due to this difference in inputs resolution. Many authors have stressed that the Budyko-type formulations are improved if adding information on the seasonal variations of climate inputs. Thus, the slightly better performance in favour of the process model might not be attributable to a better use of vegetation information.

RESPONSE: We agree with this. We certainly do not propose that better use of vegeta-

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tion information is the reason for the (only slightly..!) better performance of the process model, indeed it may very well be the use of temporally more detailed climate data. This does not affect our conclusions however. Key here is that the parameter values of the fitted Zhang curves for controlled experiments vs. for mixed catchment data produce models that are in apparent contradiction. The process model shows that this contradiction need not be there and is probably at least partly a statistical/methodological issue.

COMMENT: The title needs to be reformulated; it anticipates one of the results of the paper

RESPONSE: We concede that a reader at first sight might misinterpret the title. In addition reviewer 3 objects to our use of the term 'top-down'. We have changed the title to "A synthetic experiment to investigate methodological issues in detecting a land cover signal in streamflow data from multiple heterogeneous catchments ".

COMMENT: In the introduction, an interesting discussion is proposed to explain what the authors call the "land cover paradox". Two kinds of studies are reported: (i) some studies based on some catchments that experienced land cover changes, allowing to address in a quite direct way the topic of the paper and (ii) some studies that explored the role of land cover by analyzing the relative behaviours of diversely land-covered catchments. The second kind of approach is an indirect mean to address the land cover change issue by replacing time (land cover modifications on a given catchment, which is referred to "control experiment") by space (different land covers from several catchments yield different catchment behaviours). The problems discussed by the authors in the paper are mainly related to the later approach and those problems are clearly stated. However, the "control experiment" has also its inherent problem since a modification of the vegetation is often associated to a modification of the soil properties caused by forestry machine. Consequently, the change in streamflow due to land cover modifications may also be attributable to soil modifications.

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RESPONSE: We agree that controlled catchment experiments come with their own problems. We mentioned several of them in Section 1 under the numbered headings, but indeed this is an additional one that we are aware of (see Bruijnzeel, 2004) and that is worth mentioning. We did so in the revised m/s (line 144-148)

COMMENT: The authors state that the Penman-Monteith (PM) equation is used to estimate actual evapotranspiration within the AWRA-L model. I am a bit confused since the classical PM equation is a potential evapotranspiration equation that does not take into account the water deficit in the soil. Do the authors make the stomatal resistance vary in time according to soil moisture in order to tune the PM equation into a simple SVAT scheme?

RESPONSE: We have clarified this in the text: "Maximum evaporation and transpiration given atmosphere and vegetation conditions are estimated using the Penman-Monteith model (Monteith, 1965). Actual transpiration is calculated as the lesser of maximum transpiration and maximum root water uptake given soil water availability." (line 273-276)

COMMENT: P.4131 l. 17 and l. 24. Is it Qsim or Qobs?

RESPONSE: It is Qsim; we have clarified this by adding: "We did not use the actually observed streamflow as this already contained measurement noise." (line 355-356)

COMMENT: P.4133 l.3 Is it really necessary to perform the test on the covariance, given the poor correlation coefficient? Is the relationship even significant?

RESPONSE: This is a fair question, and indeed a reason why we performed this particular experiment. The purely statistical answer would be that r=0.44 is significant at a high level of confidence for N=278.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 4121, 2011.