

## ***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.***

**Anonymous Referee #2**

Received and published: 6 June 2011

This paper questions the relevance of classical methodologies to assess the influence of land cover on runoff. Two modelling approaches are explored, one being based on the Budyko framework, and the other being more process-oriented. The issue is clearly relevant for publication in HESS. The database is impressive and thus allows to reach some general conclusions. I found interesting the idea to compare the outputs of two conceptually very different modelling approaches and the tested hypotheses are quite interesting. However, I do not share the optimistic view of the authors and I do not think they have reconciled the land cover paradox at all.

C1991

### Major comments

#### 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 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. 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.

#### 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.

### Less major comments

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

C1992

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.

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?

P.4131 I. 17 and I. 24. Is it  $Q_{sim}$  or  $Q_{obs}$ ?

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

---

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