

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 #3

Received and published: 19 June 2011

This paper is well written in the usual sense, but I am not supportive of its publication. In my opinion the paper has fundamental flaws in its conception and methodology. The conclusions are not supported.

I would organize my objections to this paper under three headings:

1) Firstly, the paper talks as if the “land cover paradox” exists and is demonstrated. I disagree with this assertion. The evidence for this must be presented in this paper (with the claim that it presents top-down analysis). Even if it may have been talked about in the literature, I am not convinced that it deserves its elevation to a “paradox”, which is

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far too much an over-dramatization. There are far too many counter-examples where land use and land cover changes over large regions have made significant impacts on water balance, e.g., Amazon basin, Upper Mississippi basin, in the island of Borneo (in both Indonesia and Malaysia).

2) If at all the impacts of land use land cover changes under mixed cover are not clear or insignificant, one of the most important causes should be ecohydrologic, i.e., the interaction between different land covers (pastures vs forests) – the extra runoff feeding to the forested regions being used by the trees, at least up to a limit. The extent of land cover changes and the nature and patterns of mixed cover must be factors that must be considered. This is not even mentioned in this paper, whereas to my mind, it must be the primary causes of the so-called paradox. The process modeling approach used to test the causes of the paradox does not consider it, and therefore incompatible to the needs of such modeling investigation (e.g., one needs an ecohydrologic model).

3) I have strong reservations (even objections) to the modeling approach used here. First of all, to call this a top-down modeling means the authors do not even understand what top-down modeling is, in the first place. The kinds of analysis that led to the Zhang curve, I will admit is a top-down analysis (I would prefer to call it an empirical analysis to extract a pattern, which is merely the first step in top-down analysis, seeking explanations for the pattern is really what top-down analysis is).

In this study, there is absolutely no data analysis at all. What is really being done is secondary analysis with the use of the Zhang curve, and associated manipulations, to see they can come up a combined model that can handle mixed land cover. In view of my opinion that the true cause of the ‘land cover paradox’ is ecohydrologic interactions between different land covers, mere superposition of Zhang curves is not going to capture the effects of these interactions.

4) Given these fundamental problems with the paper, the remainder of the analysis and discussion is merely adding confusion and obfuscation for a fundamentally flawed

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conception of the analysis.

I am afraid that I have to recommend rejection of the paper in its present form.

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

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