Hydrol. Earth Syst. Sci. Discuss., 8, C3460–C3463, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C3460/2011/

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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Received and published: 14 August 2011

INTRODUCTION: We thank the reviewer for his/her frank assessment. Below we provide a detailed response, 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: 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 far too much an over-dramatization.

RESPONSE: We are not sure which view to respond to here: the reviewer initially disagrees that the paradox we defined exists, then appears to concede that it exists but should not be called something as elevated and over-dramatized as a paradox, and finally (in his/her next comment) has decided on the primary cause for the paradox. To be sure, the word paradox is by no means an elevated or dramatic concept, but a convenient word that is more or less synonymous with 'apparent contradiction' (see e.g. Webster's or the Oxford Dictionary). It is useful to describe a situation where one is logically challenged by facts.

COMMENT: 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).

RESPONSE: the paradox we define in the manuscript does not invoke these types of studies and hence this seems a tangential point. On that tangent, however, we agree that several such studies have been published, but we are also aware that many of them have later been shown to have mistaken climate variability impacts for land use impacts. Without references we cannot assess whether this may or is the case for the studies the reviewer has in mind.

COMMENT: 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).

RESPONSE: The reviewer asserts that any causes for the paradox we address 'should be ecohydrological'. We demonstrate that statistical-methodological causes alone are, in principle, sufficient to explain the paradox. However we certainly agree there can also be ecohydrological causes. Several were indeed discussed (see line 112-148, 600-635, and 653-655), although in the absence of evidence we do not feel as confident as the reviewer in asserting that they 'should' be the cause.

COMMENT: 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.

RESPONSE: We use the terms 'top-down method' and 'top-down analysis' respectively. Top-down modelling would suggest that these models are used to make predictions, which is not the case in the context of this study. The reviewer appears to refer to some generally accepted and detailed definition of what constitutes a 'top-down' method. We can find no evidence of that. The usage we adopted (through reference) is that introduced by Klemeš (1983) and Sivapalan et al. (2003). We certainly concede that both papers apply the term quite loosely, alternately referring to e.g. scale of analysis, origin of theories, model complexity, or number of (calibrated) parameters. In our interpretation, both papers read more or less as manifests that identify, in hydrol-

ogy, the contrast between reductionist and holistic approaches or positions also found throughout other areas of science. We used the term 'top-down' because it seems favoured in hydrological literature and was professed to by several of the studies that used Budyko's model for the type of analysis we describe.

COMMENT: Given these fundamental problems with the paper, the remainder of the analysis and discussion is merely adding confusion and obfuscation for a fundamentally flawed conception of the analysis.

RESPONSE: We would need more detail to be able to respond to this comment in a more useful way.

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