

Interactive comment on “Characterizing temporary hydrological regimes at a European scale” by M. J. Kirkby et al.

M. Sivapalan (Referee)

sivapala@uiuc.edu

Received and published: 19 June 2011

Characterizing temporary hydrological regimes at the European scale M. J. Kirkby, F. Gallart, T. R. Kjeldsen, B. J. Irvine, J. Froebrich, A. Lo Porto, and the MIRAGE team

This is a brilliant paper that opens the way for much thoughtful and insightful analyses and predictions in the future, but I am afraid the authors are under-selling the importance of this work.

Essentially the paper uses a simple water balance model that partitions the precipitation into fast and slow runoff and of sufficient complexity to capture the climate and landscape controls on this partitioning.

They use this model to derive (numerically) the monthly flow duration curves (thus focusing on slow runoff) on a grid across the whole of Europe. In this way, they are able to map the regional variations of certain quantiles of the flow duration curve that are important for ecosystem health.

This leads to a classification system that opens that way to generating specific hypotheses that can be tested using more detailed studies across a gradient (climatic, geologic, human-impact etc.). I am really supportive of this kind of model-driven classification studies – in fact this work encourages with my own efforts at developing a classification of flow duration curves using a similar process-based model.

The model they use is fairly simple, but in my opinion is able to capture the first order controls of climate and landscape properties. Clearly, the model may not be able to capture the role of landscape factors especially at small spatial scales; however it should be adequate for large scale (regional) studies such as the one presented here.

However, in spite of its relative simplicity, it is nevertheless a “rich” model in the sense of allowing for vegetation cover and root biomass to evolve in response of the water balance and this impact on the soil hydraulic properties; in this sense I recognize an early version co-evolution model that is suitable for climate impact studies. In other words, it is a model that is capable of providing more robust first order estimates of climate change impacts than the more sophisticated but much less robust (highly uncertain) earth system science models.

I therefore congratulate the authors for paving the way for sophisticated and thought studies to be carried out in the future.

While I am very sympathetic towards publication of this paper in HESS, I have two suggestions towards improved presentation of the paper.

(1) The presentation can be improved – much of the abstract and the bulk of the text of the paper come across as rather cryptic. It could benefit from more detailed de-

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scription that brings out the messages better. The importance of the paper for dealing with change, and co-evolution of hydrologic and ecological and pedological systems must not be under-sold. (2) Define monthly flow duration curves; I believe there is a good mapping between the monthly flow duration curves and the “regime curve” (mean monthly variation of runoff). (3) I agree with the focus on the monthly flow duration curves, but perhaps the paper can refer to recent activity on explaining the physical basis of the flow duration curves (Botter et al., 2009; and Muneepeerakul et al., 2011, both in WRR).), and the work of Yokoo and Sivapalan (2011, HESSD) that shows the connection between the slow part of the FDC and the regime curve.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 4355, 2011.

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