The manuscript reports a rather straightforward and simple statistical analysis of the dependence of base flow recession constants on a suite of climate and topographic variables. The "new" material relates to the focus on tropical regions. Regionalization via regression hasn't proven to be that great an approach (e.g., Fernandez et al. 2000) but it may be an alternative without competition for global models as suggested in the Introduction of the current manuscript? What we do know is that the plethora of previous studies show that statistical relationships are likely to be weak at best [e.g., see articles cited by Hall (1968) as well as more recent work cited in the current manuscript], so it makes sense to take some care in treating the data.

How good are the estimates of the individual recession constants? Vogel and Kroll (1996) point out some of the statistical niceties for estimating recession constants. Isn't their advice useful in organizing a statistical analysis for recession constants? It is pretty well known that getting stable estimates of k_{bf} isn't that easy (e.g., Sujono et al. 2004). Are the analyses described in the manuscript solid?

Can't the selection of "independent" variables be informed by theory? There have been so many studies done relating climate and physical characteristics to baseflow recession that it might be more interesting to examine some indications provided by theory rather than just present a blind statistical analysis. For example, the article by Zecharias and Brutsaert that is cited in the current manuscript suggests the use of only three morphometric variables. Or, Furey and Gupta (2000) suggest that drainage density squared is a better variable than just drainage density.

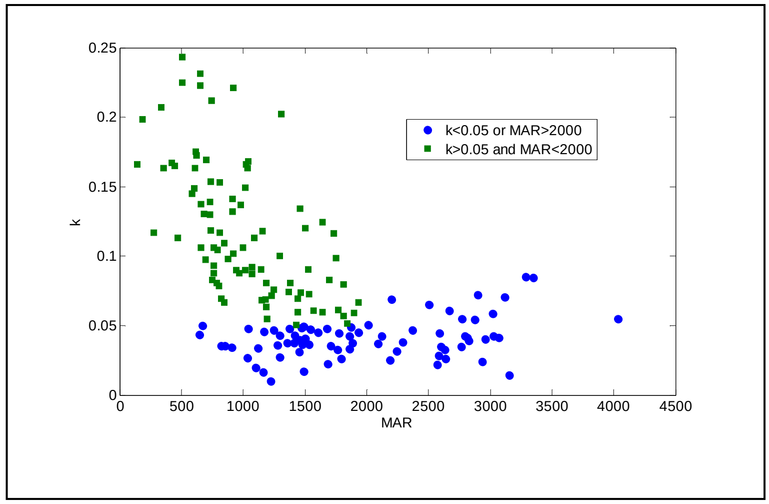
Because of correlation among the independent variables, why not use multivariate techniques to enhance the analysis? Zecharias and Brutsaert used factor analysis, for example, and Detenbeck et al. (2005) employ principal components analysis to reduce dimensionality. Although not a rigorous result, an eigen analysis of the correlation matrix reported in Table 2 of the present manuscript indicates that the first eigenvector explains almost half of the variance. Thus, it may be possible to get a better result than the one reported by including two or three eigenvectors in a regression rather than just MAR and AI.

Might dividing the data into groups provide clearer relationships? For example, it might make more sense to do separate regressions on the basis of lithology rather than lump everything together (e.g., Knisel 1963). Also see the cluster analysis and CART used by Detenbeck et al.

Other comments.

A richer discussion of some of the results may be in order. "In general, higher (faster) recession coefficients were observed for drier and flatter catchments." The 'flatter' part of this does not make intuitive sense. Is this a result using slope per se or is it for the "rainfall weighted slope", which looks much more like a climate proxy than slope? If it is indeed for slope per se, why are the results different than those of, for example, Mwakalila et al. (2002)? [In any event, clarification should be added to remove the ambiguity of how slope and rainfall-weighted slope were or were not used.]

Another discussion point might be why an exponential model (or power model) for the regressions is better than a threshold model. Could the RMSE be improved if, for example, two linear segments were used (e.g., see Figure)?



Finally, the Conclusions Section does not have any conclusions; it is a rehash of several of the points already made in the manuscript and repetition serves no useful purpose. It should be deleted.

Papers cited

Detenbeck et al. 2005. *J. Hydrol. 309*:258-276.

Fernandez et al. 2000. *Hyd Sci. J. 45*:689-707.

Furey and Gupta 2000. *Wat. Resour. Res. 36*:2679-2690.

Hall 1968. *Wat. Resour. Res. 4*:973-983.

Knisel 1963. *JGR 68*:3649-3653.

Mwakalila et al. 2002. *J. Arid Environ. 52*:245-258.

Sujono et al. 2004. *Hydrol. Proc. 18*:403-413.

Vogel and Kroll 1996. *Wat. Resour. Mgmt. 10*:363-370.