We would like to thank the two anonymous referees and Dr Anand Tularam for their helpful comments and suggestions which will be used to improve the final manuscript. Below, we respond to each referee's comments. Technical corrections, editing and minor comments were accepted and will be modified in the manuscript accordingly.

Response referee #1

Comment 1

Since the base flow recessions in many catchments have been found to be nonlinear, the authors may need to assess that for the study catchments. At least, the authors should show some details on the estimation of k_{bf}, such as the fitness of the linear model for log(Q) versus t, the values of ε_{MRE}, and the variance of the recession constant from many observed recession segments.

Response

We recognise the importance of assessing the linear or non-linear behaviour of the studied catchments for scientific purposes, but for the purpose of this study, the assessment of non-linearity of catchments was not an objective. Catchments were treated as linear reservoirs to obtain recession coefficients keeping in sight that estimates of linear reservoir parameters were needed in global and large scale hydrological modelling studies cited in the manuscript. Models used in those studies make use of such representation in their groundwater flow conceptualisation.

We will include more details and summary statistics of k_{bf} and ε_{MRE} to illustrate and clarify the variance and stability of recession coefficients as a function of changing window size T_{QF} and. In addition, examples of this variance in catchments with different climatic and terrain characteristics will be added.

Comment 2

The regression analysis is based on single variable instead of multivariable analysis, such as MAR or AI. MAR and AI together only explain 49% of the variance. Other variables which are not included in the analysis may explain the rest of the variance, and multivariable regression will improve this. It is necessary for the authors to discuss this for future work. The authors did an analysis of clusters of catchments of smaller size. To improve the performance of regression, cluster analysis can be conducted first; then the regression can be done for each cluster.

Response

A multivariate regression analysis was performed (Section 4.3, page 4070 line 10). From all terrain and climatic attributes that explained residuals of the initial regression, only catchment elongation was weakly correlated. Adding this covariate and obtaining a two term equation only marginally improved results. Although a simple multivariate regression equation was used in the end, much of the variance of recession coefficients was explained by just one covariate and a simple exponential model. The technique used can and will be enhanced in future work; as the other referee suggested other techniques e.g. principal component analysis can be used to improve the results by reducing the number of possibly correlated variables prior to regression analysis. By doing this a more sound justification for dropping some of the covariates will be provided. This explanation will be added into the discussion section.

Clustering was performed based on geology by using rock type and age from the digital Generalized Geological Map of the World (Canadian Geological Survey, CGS, 1995). A proxy for drainage potential was obtained following the procedure of Döll and Fiedler (2008). This work was not reported due to somewhat disappointing results with very low correlations for the clustered classes. A second proxy for drainage included in the manuscript based on WHYMAP was also used to this end. Results did not substantially improved and were also left out of the manuscript. The geology proxy used in the analysis, aquifer drainage potential, did not reveal any pattern (Fig. 3) and its low correlation value indicated no influence on recession coefficients for this particular dataset. One would expect that geology and associated derivatives play a central role in groundwater recession rates. This may well be ascribed to the lack of detailed geology at a global scale and the variety and geographic extent and distribution of catchments use in the study. More detailed data may improve the results, we are looking into areas with better geological data to repeat the analysis and check if influence of geology can be observed as reported in other studies.

Comment 3

Catchments with dams, substantial LCC and snow cover have been excluded for analysis. Are there any other human activities in the study catchments such as groundwater pumping and urbanization which can also affect the base flow recessions?

Response

Many thanks for raising this important point. We will include the following explanation and citations in the manuscript: The study aimed to use first or second order catchments with no or low human water –related productive activities or extensive urban areas occurring within catchment boundaries; however these criteria had to be more flexible due to the lack of good streamflow data for this particular analysis. The 167 catchments were initially screened for urbanisation and irrigation using GLOBCOVER land use data. We re-revised the urban areas in the dataset using the MODIS 500-m map of global urban extent (Schneider et al., 2009). Only 10 catchments in the dataset had urban areas of somewhat considerable size but never more than 5% of total catchment area. Thus these were considered unregulated for this study. No catchments had large irrigation areas within catchment boundaries so it was assumed that any groundwater extraction would be small and have little impact on recession coefficients.

Comment 4

Lines 16-18 in page 4061, "The use of drainage ...". Why? More discussion or reference is necessary.

Response

We will rephrase this sentence and add information (see below) to make the message clear. The Brutsaert and Nieber (1977) procedure is one of the few analytical methodologies to obtain aquifer parameters from hillslope to catchment scales. Using this theoretical approach, Zecharias and Brutsaert (1988) advanced a proportionality relationship between recession coefficient and aquifer characteristics:

$$k_{bf} \propto \frac{KD\alpha}{YL}$$

where K is hydraulic conductivity, D is aquifer thickness , α is slope, Y is storativity and L a characteristic flow path length.

Many of these aquifer parameters are not readily available in the tropics; in particular data on aquifer hydraulic conductivity and thickness are sparse and scattered. In addition, point measurements do not capture the large horizontal and vertical heterogeneity of aquifers at the "grid scale". Thus the use of drainage theory to estimate recession coefficients is questionable because (not "also" as in the manuscript page 4061 line 17) is hindered by the lack of data or their uncertain quality when these are sparse, scattered and cannot be considered representative of large catchments, as is the case in this study.

Response referee #2

Comment 1

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?

Response

The method used to obtain recession coefficients treats the catchment as a linear reservoir. For the part of the recession curve that is deemed as baseflow (5 days after the hydrograph peak) it optimises k_{bf} using the relative agreement between estimated and observed flows. In this sense it doesn't use absolute streamflow values, which could bias the results, as in other techniques to estimate recession coefficients (e.g. regression techniques using least squares).

With regards of k_{bf} stability, please refer to response to comment 1 of the other referee.

Comment 2

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.

Response

The selection of climatic and terrain attributes was indeed based on results from other studies that demonstrated correlation with recession coefficients or other baseflow related parameters (page 4065 line 9). It was considered necessary to investigate the correlation of as many climatic and terrain attributes due the geographical extent and differences in climate and geology of the catchments used in the study. Other studies may not make use of climatic indicators because of the proximity of the catchments used therein, or only made use of better and more detailed information on soils, geology or perennial streams which would define characteristics such as soil drainage rates, aquifer hydraulic conductivity or drainage density respectively.

Comment 3

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.

Response

The other referee also observed this issue (please refer to response to comment 2). We think that the high multicollinearity of climatic attributes will result in no further explanatory power (expressed as reduced variance of residuals after the initial regression) if more than one climatic attribute is used. Although we recognise that a more sound technique such as factor or principal component analysis might be used to justify dropping covariates from the analysis. A comment could be added to mention this.

Comment 4

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.

Response

This was tried unsuccessfully. We refer to the response to comet 2 referee #1 for details.

Comment 5

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.]

Response

We will clarify this and add more details in the manuscript about the use of rainfall-weighted slope. Rainfall-weighted slope and not slope per se was used throughout the study. We think that at the catchment scales used (mainly 200–1000 km^2); there are areas that are more hydrologically active than others due to differences in rainfall. These areas will have more impact in the generation of baseflow and thus on recession coefficients. By multiplying each pixel from a catchment slope raster by the catchment normalised rainfall raster, we ascribe the slope pixels with higher rainfall more importance. This computation has marked effects on catchment with high rainfall spatial variability and large areas (>500 km^2 or less when catchment orographic effects were captured in the rainfall data). Slope or rainfall-weighted slope might not be the best descriptor for smaller catchments if the purpose is to describe basin relief (e.g. Post and Jakeman, 1996) but is deemed adequate for the complex topography of larger catchments. The negative correlation between recession coefficients and rainfall-catchment weighted slope is counterintuitive; what common sense tells is that rugged catchments drain quicker than flatter ones (e.g. Post and Jakeman, 1996). However we think that this effect has less relevance when catchment areas are larger and other effects such as climate or complex topography/geology override the effects of slope, this was also observed in Post and Jakeman (1996) but their results were not conclusive. A similar negative correlation was reported in Van Dijk (2010) for 183 catchments in mainly temperate Australia.

On the other hand Mwakalila et al. (2002) and Brandes et al. (2005) found positive correlations between catchment slope (and proxy) in their studies, although neither was statistically significant. Their samples were small (n=12 and n=24) and a visual inspection of slope (or proxy) vs. recession coefficients (or baseflow index) scatters did not reveal clear patterns.

We agree that the 'flatter' assertion is not correct and it will be removed. The higher recession coefficients occurred in drier catchments and are probably the result of fast-draining and perched groundwater close to streams.

<u>Comment 6</u>

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)?

<u>Response</u>

A segmented regression could be considered if there are sound hydrological arguments to justify a threshold or breakpoint for these particular dataset, which we don't think is evident.

<u>Comment 7</u>

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.

<u>Response</u>

We agree with the referee's suggestion. The Conclusion Section will be renamed Summary and Conclusions and the main findings will be presented in bullets.

References

Brandes, D., Hoffmann, J. G., and Mangarillo, J. T.: Base flow recession rates, low flows, and hydrologic features of small watersheds in Pennsylvania, USA, Journal of the American Water Resources Association, 41, 1177-1186, 2005.

Doll, P., and Fiedler, K.: Global-scale modeling of groundwater recharge, Hydrology and Earth System Sciences, 12, 863-885, 2008.

Mwakalila, S., Feyen, J., and Wyseure, G.: The influence of physical catchment properties on baseflow in semi-arid environments, Journal of Arid Environments, 52, 245-258, 10.1006/jare.2001.0947, 2002.

Post, D. A., and Jakeman, A. J.: Relationships between catchment attributes and hydrological response characteristics in small Australian mountain ash catchments, Hydrological Processes, 10, 877-892, 1996.

Schneider, A., Friedl, M. A., and Potere, D.: A new map of global urban extent from MODIS satellite data, Environmental Research Letters, 4, 10.1088/1748-9326/4/4/044003, 2009.

van Dijk, A.: Climate and terrain factors explaining streamflow response and recession in Australian catchments, Hydrology and Earth System Sciences, 14, 159-169,

Zecharias, Y. B., and Brutsaert, W.: recession characteristics of groundwater outflow and base-flow from mountainous watersheds, Water Resources Research, 24, 1651-1658, 1988.