Anonymous Referee #1
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General comments:

This paper attempted systematically explaining hydrological diversity of watersheds in the United States in terms of regime curves and flow duration curves. Toward the goal, authors specified dominant and necessary hydrological processes of watersheds by means of a simple hydrological modeling scheme in which model structures are reasonably determined introducing by the "downward approach", the Akaike Information Criterion, and an automatic parameter calibration scheme. Secondly they showed spatial patterns of dominant/necessary hydrological processes. Then they explained the patterns from the viewpoints of climate, potential hydrological processes, and vegetation activity of the watersheds in different regions.

I have to admit that there could be potential improvements in their process modeling, but their figures on the spatial patterns on dominant/necessary hydrological process and their explanations on the patterns along with the climatic aridity and seasonality seems very interesting and I suggest accepting this paper for publication in HESS after necessary revisions.

We appreciate the reviewer's comments and suggestions. We have made our efforts to address these comments and have indicated the specific changes. We hope this will be adequate.

Specific comments:
1. P.7040, L.10-11.
Why did you select this algorithm among other algorithms? How does this algorithm separate base-flow from total flow? Your brief explanations on these points may be helpful for readers.

We chose the Lyne and Hollick algorithm for baseflow separation is because it is one of the most widely used methods for hydrograph separation (Welderufael and Woyessa 2010), and is easy to implement. Similar work presented by Troch (et al. 2009) suggested that the hydrologic partition was not strongly sensitive to the baseflow separation method. We have added the equation in the manuscript as reference. We thank the reviewer for the suggestion.

2. P.7044, L.19-20.
Why did you use the same mean residence time? What would be the possible effects? Can you justify it? Please explain for the above point.

Although we separated the fast flow from slow flow because of their different generation mechanisms and flow paths (most fast flow is surface runoff while slow flow is mainly subsurface runoff), they are routed together in the river network once they enter the river channel. Therefore, we use the same mean residence time to represent the time they *spend in the river network system. We have also added a brief explanation in the manuscript. We hope the reviewer finds this explanation satisfactory.*

3. P.7047, L.21.
Do you need calibrations for Tmin and Tmax? Please add explanations.

According to Jolly (et al., 2005), the lower soil temperature threshold of -2^{o}C and the upper soil temperature threshold of 5^{o}C are chosen to cover a wide range of species. This soil temperature range was then converted to an air temperature range of -5^{o}C and 10^{o}C based on measured soil and air temperature (Thompson et al., 2011).

4. P7048, L.20-21.
I suggest citing an original or suitable literature on the AIC.

Thank you for the suggestion, we have now cited the following literature:
Akaike, H.: A new look at statistical model identification, IEEE T. Automat. Contr., AU-19, 716-722, 1974.

5. P.7050, L.18.
We cannot clearly understand the meaning of "chain". A brief explanation would be helpful for readers.

We use the Markov Chain Monte Carlo (MCMC) algorithm to sample parameters, which samples parameters near the previous step's parameter set. If the new set is calculated to have a larger likelihood value, it will be saved in the list. Otherwise, the algorithm will continue searching until the next available parameter set improves upon the largest likelihood. In this way, the 500 parameter samples saved are near each other and are searched one by one, like traversing the links of a long "chain." We have rephrased the description in the hope that the idea is now explained more clearly.

6. P.7051, L.7-8.
SQobs and SQsim suddenly appear in the main text without appearing in Eq.(16). So we reader are forced to search where they are. Hence I suggest closing the sentence before this part and defining them in a difference sentence after explaining Eq. (16) to calculate SQobs and SQ sim. Or, I would recommend explaining Eq.(17) first then you introduce Eq.(16) followed by the explanations of variables used in the equations.

Thank you for the recommendation; we have now moved Eq. (17) ahead and introduce Eq. (16) afterwards. This way the appearance of SQobs and SQsim is not sudden.

7. P7053, L.24-25.
Why can you claim that "This discrepancy was attributed to the absence of saturation excess runoff"? I suggest you explain more.

We agree with the reviewer that a brief explanation could make the model development procedure more logical and convincing. We have explained this claim in the manuscript as follows, and we hope the reviewer finds it adequate.

Studies examining the impact of the water table on the rainfall-runoff response (Lana-Renault et al., 2007; Li et al., 2011) have shown that during the wet season, the hydrological response could be more dependent on the water table level than the precipitation characteristics (depth and intensity). Along with the influence of the rising water-table level, the dominant flow generation mechanism would then switch from infiltration excess flow to saturation excess flow. The diagnostic plot (not presented here for brevity) shows high, steady soil moisture storage during late winter and early spring, suggesting the relatively high runoff coefficient during these months could be related to the saturation runoff caused by higher water-tables.

8. P.7054, L.10-11.
Why can you hypothesize "they could be reduced by adding canopy interception"? I suggest you explain more.

We agree with the reviewer that a brief explanation is in order so that this may be more clear to the reader; we have explained this claim in the manuscript as follows. We hope the reviewer finds it adequate.

It has been shown that interception could have a significant impact on the water cycle (Beven, 2001; Savenije, 2004); evaporation from intercepted water could reach 35% of total rainfall in wet catchments and over 40% in dry areas (Calder, 1990). This influence can then affect the infiltration, antecedent soil moisture, and flow generation (Keim, 2006). Given the high proportion of vegetation coverage in those catchments, the interception mechanism should not have been ignored. Therefore, we added the interception component, hoping it could help adjust both the fast and slow flow.

9. P.7056, L.11-12.
Are there any reason for selecting MSE=0.53 for the criterion? A brief explanation would be helpful.

A manual screening on the comparison of predicted RCs and observed RCs suggested MSE=0.53 as the criterion, catchments with MSE less than 0.53 are considered satisfactory where the predictions are capable of capturing the first order characteristics like seasonality, flow peaks, the timing of the wetting and drying periods and so on. Catchments with MSE larger than 0.53 may fail in predicting the timing of the peak or only capture one flow peaks in bimodal catchments.

10. P.7057, L.1-23.
These model-based interpretations of your results are very interesting but I would suggest citing existing literatures that support or deny your interpretations. I believe such citation would enrich your interpretations and you would be able to convince the readers more.

We agree with the reviewer that increased literature support would help add credibility to the interpretation. We have added the following literature in the manuscript; hopefully the reviewer finds it adequate.

α*: research about interception loss has shown that it can have a significant impact on the water balance, especially on evaporation (Liu, 1997). In addition, forests are able to intercept more rainfall than grasslands (Deguchi et al., 2006); among the forests, coniferous forests tend to retain more rainfall than broad-leaved forests (Marin et al., 2000). These insights are consistent with the calibration results.*

Se, Sb2: due to the interaction between vegetation and climate, soil depths adapt to the environment. The increasing trend of soil moisture capacity from east to west may be related to climate seasonality (Samuel et al., 2008): in the eastern, humid catchments, where rainfall arrives throughout the year, the high moisture storage and slope help drain quickly, leading to a smaller quantity of storage; in the center of the continent, with moderate seasonality and flat topography, the Midwestern catchments are usually characterized by deep soils and stronger soil moisture retention characteristics (Endres et al., 2001; McIsaac et al., 2010), near the west coast, due to the strong seasonality in P being out of phase with ET which overcomes the slope drainage, the soil moisture storage tends to accumulate during the wet season, leading to higher overall storage.

tu: we attribute the regional pattern of the subsurface flow drainage time to the catchments' topographies (slopes), which show a similar regional pattern with respect to tu. This is consistent with the findings of McGuire,et al. (2005) in seven catchments with diverse geologic and geomorphic conditions: instead of basin area, the residence time is strongly related to terrain indices indicating flow path distance and gradient.

Deguchi, A., Hattori, S., Park, H., The influence of seasonal changes in canopy structure on interception loss: Application of the revised Gash model, J. Hydrol., 318, 80-102, 2006.

Liu, S.: A new model for the prediction of rainfall interception in forest canopies. Ecol. Model., 99, 151–159, 1997.

Marin, T.C., Bouten, W., Sevink, J.: Gross rainfall and its partitioning into throughfall, streamflow and evaporation of intercepted water in four forest ecosystems in western Amazonia. J. Hydrol., 237, 40–57, 2000.

McGuire, K. J., J. J. McDonnell, M. Weiler, C. Kendall, B. L. McGlynn, J. M. Welker, and J. Seibert (2005), The role of topography on catchment-scale water residence time, Water Resour. Res., 41, W05002, doi:10.1029/2004WR003657.

G. F. McIsaac, M. B. David, and C. A. Mitchell: Miscanthus and switchgrass production in Central Illinois: impacts on hydrology and inorganic nitrogen leaching, J. Environ. Qual., 39, 1790-1799, 2010.

Samuel, J. M., M. Sivapalan, and I. Struthers: Diagnostic analysis of water balance variability: A comparative modeling study of catchments in Perth, Newcastle, and

Darwin, Australia, Water Resour. Res., 44, W06403, doi:10.1029/2007WR006694, 2008.

11. P.7059, L.19, P.7062, L.17-18.
Why these catchments are easily modeled by the base-model? Such catchments would have vegetation. Your explanations must be helpful even with potential reasons.

We agree with the reviewer that there is significant vegetation coverage in these southeastern catchments, though vegetation is less dense than in the Appalachian Mountain catchments. One reason these catchments are easily modeled is because of their seasonality. The catchments in Florida experience a wet season from mid-summer to early fall (Fig. 1(i)); they receive abundant rainfall that is caused by frequent convective activity as well as the occasional tropical stormssimilar to the monsoon seen in Asia (Fernald and Purdum, 1998). The catchments in Georgia also display seasonality in precipitation; they receive heavy rainfall during winter and spring when the evapotransipiration rate is quite low, thus enhancing the seasonality observed in streamflow generation (Opsahl et al. 2007). Therefore, like the western coastal catchments, catchments in Florida and southern Georgia are also easy to model via the base model as the seasonality in climate overwhelms the other factors. In addition, the pehnology influence is mitigated in these southern catchments since the duration of vegetation coverage in those southern catchments are much longer than the Appalachian Mountain catchments due to their southern latitude.

12. P.7064, L.7.
“four level 3 models”. This expression sounds not straightforward tome. A more careful expression is preferable.

We have changed this to the following, in the hopes the reviewer finds it appropriate now:

Figure 14 presents the observed FDCs of total flow based on simulations with the complete model, as well as by four “reduced” models in which one of the four processes from the complete model is removed, leaving only three processes. The parameter set, optimized for the full model, is applied to all five models.

13. P.7065, L.4-P.766, L.22.
I think a comparison with Keppen’s climate classification map would potentially enrich or support your discussion. But this is not mandatory. I am just suggesting it if it is easy for authors.

We agree with the reviewer that comparison with Köppen’s climate classification map would help enrich this paper and have added the following paragraph in the discussion:

In the eastern catchments, from north to south, as temperature increases, snow influence ceases and vegetation influence declines, the requisite processes decreases from full model to base model from the Dfb (snow, fully humid, warm summer) class to Cfa (warm *temperate, fully humid, hot summer) class in the Köppen's climate classification. In the western continent, Köppen's climate classification becomes more scatter, as well as the model class. In the western mountainous forested catchments, which fall in the snow steppe climate class (Dsa), the snowmelt is the most important process; while in the northwestern catchments on the coast, which belongs to the warm temperate steppe climate class (Csb), the base model is sufficient due to the high seasonality. In the southwestern arid catchments (Bsk: arid steppe cold arid), more complex models are needed.*

We appreciate these technical comments and have made the suggested changes in the revised manuscript.

Technical corrections:
1. P.7048, L.15.
What the MCMC stands for? Please write in full then use its abbreviation in parentheses.

2. P.7049, L.16-17.
I think commas would be necessary in front of "given" and after "input" for readability.

3. P.7052, L.2.
"The" may be necessary in front of "AIC".

4. P.7052, L.6.
Please consider to insert a comma after "discharge".

5. P7059, L.22, P.7065, L.20.
"snowmelt" should be "Snowmelt".

6. Figs.1, 2, 5, 6-14.
The font sizes of these figures are too small to read for me. I suggest enlarging the size bigger in publication.

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