Reply to Dr. D. A. Post

We really appreciate Dr. D. A. Post for reading of our manuscript and constructive comments.

1. Overall, this is a good paper, and a useful addition to the literature on PUB. My main comment is that the authors demonstrate that the baseflow index is of key importance in identifying the characteristics of the FDC (although this is not a particularly surprising result). The authors should note of course that streamflow is required (at least for some period of time) in order to calculate the baseflow index, so the results are of limited in use in regionalising to truly ungauged catchments.

Yes, there is still a long way to go to apply what we found in this study to ungauged catchment and to reconstruct and/or regionalize duration curves. In this study, baseflow index, which was related to shape parameters of fitted duration curves, is not readily assessable for ungauged basins. This limits the capacity of these relationships for practical applications. However, the main objective of this study is to explore the physical controls of regional patterns of flow duration curves from a statistical perspective. At this stage we are not interested in regionalization of flow duration curves. Our conclusions can be benefit for reconstructing and/or regionalizing duration curves in ungauged basins, although some correlations cannot be directly applied.

2. Secondly, I do not think that the log-log curves chosen to present these data are the best choice. It is making the differences in the tail of the distribution look very large indeed, whereas the numbers involved are quite small. In contrast, the author claims that the fits are 'slightly underestimated' for high flows in most catchments. While this looks to be true on the graph, it is only the log-log plot that makes it look this way. I suspect that some of the high flows are being fit very poorly indeed.

It is difficult to show variation in the shapes across different climate and landscapes and differences in both high and low segments in one figure. In Figure 3, the vertical axis was in log space; as a result, small deviations in the low flow show much larger than they would in linear x-y axes. The horizontal axis was in a normal probability deformation to emphasise the differences in high and low flow tails. Furthermore, the Nash-Sutcliffe coefficient and goodness of fit were used to statistically quantify the differences between observed and fitted *duration curves. As show in Figure 4, fitted duration curves in some catchments were not very good, of which Nash-Sutcliffe and goodness of fit were smaller than 0.8.*

3. This statement is returned to in the conclusion, stating that 'the lower tail of the FDCs…seem especially difficult to capture'. I'm not sure that the authors have demonstrated that the tails are more difficult to capture than the peaks (at least not using Figure 3), although I suspect that this is in fact true.

There are three reasons for difficulties in fitting the lower tail of the FDC, including (1) observed duration curves contain many very small values since runoff time series was in depth (mm); (2) fast and slow flow separation algorithm resulted in many small values; (3) the fitted duration curves must approach zero when the probability of exceedance approaches 1-α; and (4) the lower tail of the duration curves may not decrease smoothly since complex runoff processes during drought periods. This complexity may come from changes in hydrological connectivity between hillslopes and streams.

4. Finally, it is not acceptable to simply refer a paper by Walsh and Lawler in 1981 to define the seasonality index. This is not a particularly common index and it needs defining here. For example, I do not understand how the seasonality index can be greater than unity as it appears to be in Fig 1 (c).

Definition of the seasonality index (SI) will be provided in the new manuscript. It is defined as: $SI = \frac{1}{\bar{R}}\sum_{n=1}^{12}\left|\overline{x_n} - \frac{\bar{R}}{12}\right|$, *in which* $\bar{R}$ *is mean annual rainfall and* $\overline{x_n}$ *is the mean rainfall of month n (n = 1, 2., …, 12). The value of SI can be larger than 1.0 if the distribution of mean monthly is very uneven as shown in Tasble 1 of Walsh and Lwaler (1981), which is pasted below.*

TABLE 1. Seasonality Index classes (when using mean monthly data)

Rainfall regime	$\overline{SI}$ class limits
Very equable	≤ 0·19
Equable but with a definite wetter season	0·20 – 0·39
Rather seasonal with a short drier season	0·40 – 0·59
Seasonal	0·60 – 0·79
Markedly seasonal with a long drier season	0·80 – 0·99
Most rain in 3 months or less	1·00 – 1·19
Extreme, almost all rain in 1 – 2 months	≥ 1·20

In the USA, Pryor and Schoof (2008) have shown that the SI around the south of California ranges from 1.0 to 1.2.

Minor comments:

Thanks for the following comments. Changes will be made in new version according to these comments

5. I assume Zhao et al. 2011 should be Zhao et al. 2012.
6. P7007, line 18. ‘the outlines of’ should be ‘outlines the’.
7. P7011, line 5, ‘approaching’ should be ‘approach’.
8. P7017, line 26 ’54 catchments’ should be ’54 years’

References:

Pryor, S. C., and J. T. Schoof (2008), Changes in the seasonality of precipitation over the contiguous USA, *J. Geophys. Res.*, 113(D21), D21108, doi:10.1029/2008JD010251.

Walsh, R. P. D., and D. M. Lawler (1981), Rainfall seasonality: description, spatial patterns and change through time, *Weather*, 36(7), 201-208, doi:10.1002/j.1477-8696.1981.tb05400.x.