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Hydrology and Earth System Sciences Discussions

Interactive comment on "Exploring the physical controls of regional patterns of flow duration curves – Part 2: Role of seasonality and associated process controls" *by* S. Ye et al.

Anonymous Referee #3

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The paper presents an extensive analysis of regime curve behavior in the US, performed by means of a downward approach based on the application of models with increasing complexity. The topic is of great interest and also the approach adopted is interesting. Obviously, due to the strong heterogeneity observed in regime curves in all the US, I believe that the authors encountered more than a few problems in trying to reduce such complexity and provide a clear overall picture from results. I also think that some more effort could be devoted in improving the readiness of the paper by clarifying the procedure adopted and the presentation of results.

Comments

1. Page 7039, lines 23-25; the authors should mention what kind of LAI maps they used (daily, monthly, algorithm used, etc)

2. Page 7040, line 5; the authors should introduce here (or before) the definition of the "regime curve" mentioning witch kind of year they used (civil, solar, hydrological. . .?)

3. page 7040, line 10; the algorithm used for separation of base flow should be described since such a separation is important for the use that they do of the different components of flow. In particular they should specify whether it is a physically based filtering or an empirical/numerical procedure.

4. In the caption of figure 1 the AI should be mentioned as part of the figure.

5. While it is clear that the curves shown in Figure 1 are chosen as representative of different areas (states) of the US, the authors should mention which are the 9 basins that were chosen and comment how far they can be effectively considered representative of the area with respect to heterogeneities eventually observed within it.

6. Page 7043, line 13; "topographically driven subsurface drainage", the authors with this definition provide an important physical characterization of the subsurface flow process. While in principle I would agree on such a definition, I would also like to know how far the authors detect the influence of topography in their results, considering that topography, as far as I understood, is not explicitly parameterized in their model.

7. Page 7044, bottom line, "e.g. catchments in California", I think that here the authors should specify "North CA".

8. Page 7047, section 2.2.5, which algorithm or method (data) was used to evaluate PET ?

9. Page 7051 Eq. 16; I do not understand this equation, the authors should please better specify how they obtained their standardized flow values.

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10. Page 7056, line 11; The authors choose 0.53 as the threshold for detecting a "satisfactory" behaviour. This choice is not really motivated, what would happen if they assumed 0.50 as a threshold ?

11. Page 7062, line 3; I believe that "self-similarity" is not properly mentioned here. I would rather say "hydrological similarity".

12. Page 7064, section 4.6; since the analysis on flow duration curves is reduced to comments in this short section, and also, results are not quite satisfactory, I definitely suggest to move the emphasis of the paper (in the title and in the abstract) from flow duration curves to the regime curves.

13. Last but not least, as an overall final comment, I believe that the authors should put more emphasis on the procedure that they use for validating their models which is suitable, in my opinion, for the purposes of the paper but need to be better addressed. The purpose of the paper is speculative and not aimed at proposing predictive models. In this perspective I believe that their model validation is a particular case of the "scientific validation" introduced by Biondi et al. (Physics and Chemistry of the Earth, doi:10.1016/j.pce.2011.07.037, 2012) as opposed to "performance validation" of predictive models. If the paper purpose lies in this second case (performance validation) I would ask for a more rigorous process of calibration\validation (first: split sample; second: recalibration of parameters when moving towards more (or less) complexity and so on). On the other hand, according to the principles of scientific validation, a good point of the paper, which is not commented at all by the authors, is the multi-objective validation that they perform by separately comparing the fast and slow flows. This procedure actually allows to detect improvements of the model performance that poorly affect the global discharge (see for example Figure 7 and figure 8) but are significant for characterizing the fast flow component and detecting the main control processes.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 7035, 2012.