

***Interactive comment on “Does the simple dynamical systems approach provide useful information about catchment hydrological functioning in a Mediterranean context? Application to the Ardèche catchment (France)” by M. Adamovic et al.***

**Anonymous Referee #1**

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General comments:

The manuscript presents the implementation of the Kirchner's methodology for describing a catchment as a “simple dynamical system” in several Mediterranean catchments in France. The implemented methodology is rather new and I support its implementation in different hydrogeological or climatic settings. This would undoubtedly contribute to its further development and identification of the possible limitations such as the ones

C4589

presented in this paper.

In my view, the most important aspect of the paper is the fact that the hydrological data from the operational network (“lower” quality data) has been used in the study. Therefore, the study might presents possible way how the “official” state hydrological monitoring network data could be used for implementing the methodology presented by Kirchner (WRR, 2009).

It is known that hydrological model performance generally decreases if there are substantial differences (errors) in the water balance; namely, the water balance presents a basis for most of the hydrological modelling efforts. If the simulation is derived directly from the mass balance (the case of the Kirchner methodology), then the water budget related problems become even more pronounced. The authors have demonstrated that the main limiting factors for the application of the methodology in Mediterranean climatic conditions is limited assessment of the actual evapotranspiration. I believe this should be more clearly pointed out in the paper.

Related to the comment above, the simplified relation that yearly  $AET = PET$  might work on a yearly basis, but might be highly questionable during different seasons. This hypothesis is in my view the main reason that the simulation results are poor during summer. AET rates were found to be substantially underestimated in cases of numerous Mediterranean catchments. This is indicated by the runoff coefficients for the summer rainfall events in the Mediterranean catchments which are extremely low (e.g. see the values reported by Llorens, 1997 (J. Hydrol); Rusjan et al., 2008 (J. Hydrol); Šraj et al., 2008 (Agr. Forest. Meteorol); Cognard-Plancq et al., 2001 (J. Hydrol); Boronina et al., 2005 (Hydrol. Process); Cosandey et al., 2005 (J. Hydrol). It would be informative to present some representative data on e.g. monthly budgets of the hydrological cycle (P, Q, PET, AET-derived as a difference between P and Q) as this would probably disclose the problems related to the water balance.

The annual assessment of AET and consequent mass balance analysis (consistency)

C4590

relies strongly on the Turc (1961) methodology. Can authors provide some information that would support the implementation of the Turc simple equation for the estimation of AET from P and T in Mediterranean climatic conditions?

The authors stated throughout the paper that the hydrological response of granite catchments is dominated by the saturation excess runoff. In terms of the conceptual understanding of the rainfall runoff formation mechanisms, the saturation excess runoff probably bypasses the catchment storage as defined by Kirchner (WRR, 2009). How then the hydrological response of the catchments presented in this paper agrees with the hydrological characteristics, where the original methodology was developed? Could this also be one of the reasons for worse simulation performance?

Specific comments and technical corrections:

1)Page 4, lines 31-32: The sentence needs grammar revision.

2)Page 5, line 26: What are the “main terms” of the water balance?

3)Page 8: How was the rainfall data consistency performed? On what temporal step (hourly, daily sums?)

4)In my opinion, table 3 contains extensive list of coefficients that are not properly addressed and consequently extremely difficult to follow in the manuscript, the results presented in the Table 3 are also not properly presented. Most of the studies in the Mediterranean catchments report highly underestimated rates of the PET compared to AET derived from P-Q mass balance.

5)Page 10, lines 22-23: What would be a “realistic” runoff coefficient for analyzed type of catchments?

6)Page 9, lines 27-28; Figure 4: What represent lines and crosses? How can AET/P in Fig. 4 range between 1.5 and 3 if the y-axis representing the AET/P ratio ranges between 0.1 and 0.7?

C4591

7)Authors show only the recession rates for catchment #1 (Fig. 5), it would be interesting to see graphically, how the recession rates (described by quadratic curve fitting reported in Table 5) differ between different catchments.

8)Page 26, line 6-22: The links between the recession curves and hydrogeological characteristics could be more thoroughly presented. How are the characteristics of the catchments reflected in recession rates? This is only roughly mentioned in the paper and would, in my opinion, need a more thorough discussion.

9)Table 10: The station names should be supplemented with catchment No. as these are referenced throughout the paper.

10)Figures 8 and 9: My impression is that there are too many curves shown in the same graph that do not provide any additional valuable information.

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C4592