March 27, 2015

To editor

To the two reviewers of the manuscript, students and Eric Gaume that left their comments.

Hydrology and Earth System Sciences

Re: Revised Manuscript "***Does the simple dynamical systems approach provide useful information about catchment hydrological functioning in a Mediterranean context? Application to the Ardèche catchment (France)***" by Adamovic M. et al.

Dear Editor,

We appreciate the efforts you and the reviewers have invested in our manuscript. We are pleased to submit a revised version in response the reviewers' insightful comments.
We have responded to reviewer comments in the discussion phase, here we just indicate point by point the way that the manuscript has been modified to respond to the comments by reviewers. The reviewer comments appear in black italic and our answers are provided in blue. References to the modified parts in the revised manuscript are underlined in blue.

Comments by Referee #1

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 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).

We thank Referee#1 for this positive appraisal of the paper content.

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.*

The issue of the assessment of actual evapotranspiration and its impact for the method application was underlined by all the reviewers of the paper (see also specific comments by Reviewer 2). Therefore this question is more discussed in the revised version of the paper (Sect. 2.2.2 (p.8 26-31), 2.3 (p.10 22-27) and in Sect. 5.1 of the discussion (p.27 21-31)).

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.

First, we would like to highlight one point which was probably not fully clear in the paper presentation. In fact we assume that $AET = \alpha_{AET} * K_c ET_0$ where α_{AET} is the scaling *AET* factor provided in Table 3 of the original paper. While this scaling factor is assumed to be constant throughout the year, hourly variation (hourly ET_0 signal) and seasonal variations (seasonal K_c) of *AET* are considered. We agree that a mean annual value of α_{AET} is probably too coarse, as strong seasonal variations in *AET* signal are expected due to the seasonal variations of ET_0 and vegetation activity. However, the Turc (1951) formula only provides annual values of *AET* and the water balance approach (*AET=P-Q*) that we used as a reference is also valid only for interannual averages. The method of Thornthwaite and Mather (1955) cited by Gudulas et al. (2013) provides monthly estimates of *AET* and could be a way to improve our simulations.

This is more clearly presented in the revised article in Sect. 2.3 (p.10 22-27 and p.11 8-14).

In order to highlight the impact of evapotranspiration in Mediterranean catchments we also calculated the average monthly budgets of the hydrological cycle (*P, Q, PET*) for the Ardèche at Meyras (#1) catchment for period 2000-2008[1]. As remarked by Referee#1 the runoff coefficients in the summer period are extremely low (in July, C=0.17 and in August, C=0.10) with reference SAFRAN ET_0 reaching its maximum in these months. There is a clear strong influence of evapotranspiration in summer periods that could be one of the explanations for the poor modeling performance in these periods.

In order to compute the *AET* at the monthly scale we use the Thornthwaite method (Gudulas et al., 2013), see Table 1[1] of the on line answer to the Reviewer.

The water balance calculation at the monthly scale suggested by the reviewer leads to inconsistent values of *AET* (negative) which is not realistic. This is also the main reason why we used an annual scale in rescaling *AET* afterwards.

[1] http://www.hydrol-earth-syst-sci-discuss.net/11/C6170/2015/hessd-11-C6170-2015.pdf

This part however we kept only in our detailed response to reviewer 1 since its place in the article would be too detailed. However, we improved our discussion by mentioning the potential use of the work of Gudulas et al. (2013) for some future papers and referring to the interactive comment in the discussion (p. 27 21-31).

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

First of all, we would like to apologize about an error in the reference to the Turc equation. The correct reference is Turc (1951). In this paper Turc presents the formula for estimating *AET* from annual average temperature and rainfall, whereas the Turc (1961) papers presents a formula to compute potential evapotranspiration depending on temperature only. This may have led to some confusion. In the 1951 paper, Turc reports an evaluation of his formula by comparing measured interannual discharge to values estimated through *P-AET* where *AET* is estimated by formula (2) of the paper with generally good performance. The considered data set covered countries all over the world. In addition, as described in the paper, one of the reasons for choosing Turc's simple equation for the estimation of *AET* from *P* and *T* in Mediterranean climatic conditions is due to equation content. Relying only on the *P* and *T* and not on ET_0, we could avoid the use of evapotranspiration and reduce uncertainty in estimating *AET*. In addition, the Turc equation is widely used in France to estimate *AET*, and thus our results can be compared to other studies.

This part was modified accordingly in the Sect. 2.3 (p. 10 22-27).

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?

Reviewer#1's comment shows that our reference to saturation excess runoff was not clear enough. In fact, in the granite and forested catchments of this region, infiltration capacity is generally very high and runoff occurs due to soil saturation (e.g. Tramblay et al., 2010). However, this saturation mostly occurs at the interface between the very thin soil and an altered bedrock, generally of larger depth, where contrasts of hydraulic conductivity can be encountered, leading to quick lateral sub-surface flow. Experiments are currently being conducted on infiltration plots to quantify the velocity of this lateral flow (see Braud et al., 2014 for their description). Therefore the main mechanism we are speaking about is quick lateral sub-surface flow which transits through the reservoir considered in the Simple Dynamical System approach. On agricultural areas, in the intermediate part of the Ardèche catchment, infiltration excess surface runoff is likely to occur (and has been observed in the field). Its contribution is also under investigation using detailed experiments (see Braud et al., 2014). At the whole Ardèche catchment scale, Adamovic (2014) tried to introduce bypass flow in the discharge simulation, but found that this only marginally improved the model performance. In addition, the optimized value of the bypass fraction was about 1%, which is very low.

In conclusion, we acknowledge that the term "saturation excess runoff" is probably not the best suited to describe the processes occurring in the Ardèche catchment but rather shallow subsurface flow caused by saturation of at interface between soil and bedrock. This latter process is consistent with the SDSA approach and is modified accordingly throughout the revised paper. The elements provided above are added to the discussion in Sect. 5.2 (p.29 7-15 and 18-31).

Specific comments

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

As the pdf version of the manuscript provides line numbers until line 25 only, we were not able to identify with certainty the above mentioned sentence. Assuming that is sentence page 10732, l 5-6, the sentence:

For our study, we need discharge data that are not influenced by human activity, as Kirchner's method assumes mass conservation.

This sentence was suppressed and the following sentences were modified in Sect. 2.2.1 (p.7 1-2 and 5-11).

Page 5, line 26: What are the "main terms" of the water balance?

Under the main terms of the water balance we consider discharge, evapotranspiration and precipitation. As we consider interannual values, change in water storage is assumed to be zero.

This was corrected in the revised manuscript in Sect. 2.1 (p.6 11-14).

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

We assume that the reviewer refers to sentence p 10734 lines 4-6. The rainfall data consistency was assessed at the hourly time step.

This was corrected in the revised manuscript within the Sect. 2.2.2 (p.8 19-20).

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.*

We took into consideration this comment and made Table 3 clearer. The table was divided into two Tables. One table provides the information about the main terms of the water balance equation (P, Q, C, AET, ET_0, K_cET_0), and the second table gives details about coefficients and corresponding rescaled variables (AET_{Turc}, T, P_{Turc}, C_{Turc}, C_n).

They are also more clearly introduced in the revised manuscript in Sects. 2.2.2 (p.8 26-31 and 2.3 (p.11 8-14) and former Table 3 has been split into two distinct tables (Tables 3 and 4) presenting respectively the water balance derived from data only, and the water balance from rescaled data.

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

We assume that the reviewer refers to sentence p 10737 l 13. We agree with the author that term "realistic" deserves better explanation. In our analysis, the runoff coefficient is another way to reflect the water balance closure in our catchment. This type of catchment is characterized by mountainous and Mediterranean influence. A low runoff coefficient indicates water losses (arid catchment). We consider as realistic runoff coefficients those ones that differ slightly from the runoff coefficient obtained at the catchment #1 where we did not used rescaled data. All examined catchments are located not so far from one another and we consider that runoff coefficients here are in the same range between 0.65 and 0.76 (see C_n coefficients in Table 2 above). Coussot (2015) extended the application of Kirchner (2009) approach to other gauged catchments of the Cévennes-Vivarais region, has obtained similar results. It shows that, once scaled data are considered, there is a quite continuous variation of runoff coefficient throughout the Ardèche and neighbouring catchments (Gard, Céze, Tarn). In addition, in the Ardèche, "naturalized" daily discharges for the gauges influenced by dams were available and the runoff coefficient obtained for those gauges are consistent with the range provided in Table 2 above. Runoff coefficients have generally higher values in the upper part (0.63-0.80) and lower values in the downstream parts of the catchments (0.60-0.57).

This part however we kept only in our detailed response to reviewer 1 since its place in the article would be too detailed. We only added the following sentence to the manuscript and Sect. 2.3 (p.11 17-19) "*We consider the rescaled runoff coefficients to be more realistic, as they are closer to those of catchment #1, where the water balance is consistent with Turc (1951) AET.*"

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?

Referee#1 is correct that Fig. 4 needs some further clarification. The values 1.5 and 3 present the values of *w* parameter and not the range of *AET/P* in Fig. 4.

This is modified in the revised version of the article in Sect. 2.3 (p.10 13-14).

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.

In the on line answer to the Reviewer[2], we provided recession curves for catchments #2, #3 and #4 as a complement to recession curves given in the original manuscript. We observe that parameters C_1 and C_2 slightly differ among the catchments with C3 parameter being similar for catchments #1, #2 and #3. A more linear relation is seen for catchment #4, which is dominated by schist and basalts geological formations. However, we keep the quadratic function as representative for all catchments. Melsen et al. (2014) concluded that a two-parameter model is reasonably able to capture high flows but not low flows. In our analysis we thus used the three-parameter model where the third parameter C_3 is essentially related to the low flows in order to capture the catchment behavior in that flow regime.

For the paper not to be too long, we decided to keep only the recession plot for catchment #1 in the final version of the manuscript, knowing that the readers are able to find the other figures in our response to reviewer 1's comment.

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.

This remark can be put close to General remark 8 by Reviewer 2 who points out that, although announced as an objective, the paper deals only superficially with the interpretation in terms of hydrological functioning of the Ardèche catchment. Therefore, we decided to change the title of the paper for “Assessing the simple dynamical systems approach in a Mediterranean context: Application to the Ardèche catchment (France)”.

However we also agree with Reviewer 1 that the interpretation in terms of hydrogeology should be better introduced. Therefore, the content of Sect. 5.2 (p.29 18-31 and p.30 3-9) was enhanced to better explain the link between the results and the catchment characteristics.

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

This is corrected in the article.

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.

We agree with Referee#1 that number of curves in Figs. 8 and 9 should be reduced. This is modified in the newer version of the article, where the range of discharge simulations is represented by a grey area. The lower and upper bounds lie within the range of “behavioral” values (see Table 10 in the original manuscript).

[2] http://www.hydrol-earth-syst-sci-discuss.net/11/C6170/2015/hessd-11-C6170-2015-supplement.pdf

Comments by Referee #2

General comments

1. Does the paper address relevant scientific questions within the scope of HESS?
Yes. The sdsa is an elegant new method and very powerful if it works. Many people try it and some succeed. It's good to get some examples in the literature showing when and where it does yield satisfactory results - and when and where it doesn't.

2. Does the paper present novel concepts, ideas, tools, or data?
Yes, this paper gives valuable new insights into the sdsa, even though it is mostly a new application of an existing method.

3. Are substantial conclusions reached?
Yes.

4. Are the scientific methods and assumptions valid and clearly outlined?
Yes.

5. Are the results sufficient to support the interpretations and conclusions?
Yes.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
Yes, although the ET and P rescaling procedure could be clarified.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
Yes.

8. Does the title clearly reflect the contents of the paper?
I think the title could be adapted to better fit the contents. You didn't investigate in depth whether the sdsa "provides useful information about catchment hydrological functioning". I can imagine that that is what you set out to investigate, but before being able to answer that question, you had to analyse if the sdsa would work at all and that's what most of the paper is about (which is sufficient). You do get back to the question on hydrological functioning a little in the discussion, but I still think it a secondary question.
I would therefore advise to change that part of the title to "yield satisfactory results" or something similar.

9. Does the abstract provide a concise and complete summary?
Yes.

10. Is the overall presentation well structured and clear?
Yes. I always like it when Subsection headers in the Methods and Results Sections are the same, so that when I am confused in the Results Section, I can easily find the explanation in

the Methods Section. You kept this symmetry nicely. (Maybe it can be further improved by choosing either "simulation" (3.2) or "simulations" (4.2) for both 3.2 and 4.2, and "Rainfall" (3.3) or Precipitation (4.3) for 3.3 and 4.3, but these are unimportant details.)

11. Is the language fluent and precise?
Yes. I found it very well-written – I never had to read sentences twice.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
Generally, yes. Some minor things:
In Eq. 18, the mean of the observed discharge Y_i^{mean} should not include the subscript
i. You could (if you like) also add "obs" to the superscript to make clear that it's the mean of the observations (although it is specified below the equation).
- In Eq. 19, you can remove the outer brackets and the brackets around the 100. Maybe also mention that the 100 is for scaling to percents.
- The Nash-Sutcliffe Efficiency is first abbreviated as NSE and later as NASH.
-c1,c2 and c3 are first in small font and later in capitals.
- Are you sure c1, c2 and c3 are unitless? I could be mistaken, but I think the units of some of these may depend on the values of others. I don't recommend going into details, but maybe you could mention it (if it is indeed true) and not say that it's unitless.
- Sometimes you use round and sometimes square brackets to indicate units, both in text, tables and figures.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
The explanation of the P and ET corrections could be shortened and clarified (see specific comments).

14. Are the number and quality of references appropriate?
Yes.

15. Is the amount and quality of supplementary material appropriate?
Yes.

We thank Referee#2, Dr Claudia Brauer for this positive appraisal of the paper content and for her constructive suggestions to improve the paper.
Regarding comment 8), we agree with Referee#2 that the title can be changed to the new one: We propose: "Assessing the simple dynamical systems approach in a Mediterranean context: Application to the Ardèche catchment (France)". However, we think that it is still interesting to keep the discussion on the interpretation on hydrological processes and geology (see also comment #4 from Reviewer 1). Other minor things given in comments #6), #12) and #13) are corrected accordingly in the revised version of the article.

Specific comments

28-23: You mention intense rainfall events in autumn. Is this before November (the start of the non-vegetated period)? In other words, did you take these events into account in your analysis?

Intense rainfall events occur in this region during the whole autumn (September to November). In order to choose the period for the HyMeX (Hydrological Cycle in the Mediterranean Experiment, Drobinski et al., 2014), Special Observation Period (SOP1) conducted in autumn 2012, Ducrocq et al. (2014) indicate that "The SOP1 field campaign took place during nine weeks from 5 September to 6 November. This period captures the peak climatological period of HPEs in the northwestern Mediterranean." However, intense rainfall events also occur during the whole November month, often triggering large hydrological response as catchments get wetter (Braud et al., 2014). In our recession analysis, events that occur before November were not taken into account, in order to avoid as much as possible the distortion of recession curves by evapotranspiration.

31-17 "Evapotranspiration is influenced by the seasonal cycle of the vegetation": The seasonal cycles of temperature and radiation also have a large influence on ET.

We agree with Referee#2 and the sentence is corrected as follows "*Evapotranspiration is influenced by the seasonal cycles of temperature, radiation and vegetation, the latter being particularly marked in the Ardèche catchment.…* ".

This is modified in the revised version of article, in Sect. 2.1 (p.6 20-21).

32-2 "which renders the study more challenging": It also renders the study more interesting, investigating if the sdsa can be used for practical (operational?) applications.

We agree with Referee#2 and the sentence is modified as "…*which renders the study challenging and interesting, as operational networks account for a large fraction of the available discharge data in many regions*".

This is modified in the revised version of article, in Sect. 2.2.1 (p.7 1-2).

32-6 "we need discharge data that are not influenced by human activity, as Kirchner's method assumes mass conservation." Human influenced catchments can still be used for mass conserving studies, as long as you have quantitative information about abstraction fluxes or hydropower reservoir storage.

We agree with Referee#2 regarding the human influenced catchments. Unfortunately, such data was not available for the specific purposes of our study. The section will be modified as follows *"…, which were obtained from the national Banque Hydro web-site (www.hydro.eaufrance.fr) and Electricité de France (france.edf.com/). Unfortunately, numerous dams and hydro-power stations are located in the upper parts of the Ardèche and Chassezac catchments (Fig. 1). These dams are also used to regulate the water level throughout the year, in particular to ensure a sufficient discharge in the river for recreational use in the summer period. Data to reconstruct natural discharge at the hourly time step were not available. Thus we had to discard several gauging stations located downstream of the* *dams in order to apply the simple dynamical system approach to data where the water balance can be closed.*

This is modified in the revised version of article, in Sect. 2.2.1 (p.7 5-11).

We also added some sentences about the applicability of the sdsa method to catchments with artificial reservoirs in Sect. 5.1, p.25 and lines 2-4.

The trouble with using catchments with reservoirs for the sdsa is that the assumption of a unique storage-discharge relation will not hold: there are many possible combinations of catchment storage and discharge because discharge depends largely on dam operations and not on catchment wetness. Of course this limitation will reduce the applicability of the method in practice.

We agree with this comment of Referee#2. This point is discussed in Sect. 5.1 (p.25 2-4).

32-13 How was discharge measured? A photo of the gauging station could be nice to get an idea of the measurement circumstances (if you like). A discharge of 200 l/s (the lowest Q in Fig. 6, multiplied with 90 km^2) can still be measured accurately at some gauging stations. Do you have any idea of the uncertainty associated with these observations? If you would be able to draw uncertainty bands around the observed discharge in Fig. 6, the reader would get an idea of how far off the model is. This is especially useful when logarithmic y-axes are used. Maybe as an estimate of discharge uncertainty, you could assume a fixed stage height measurement error and see how it propagates in the stage-discharge relation (just an idea).

Discharge is measured using stage measurements and stage-discharge relationships established using gaugings. The configuration of the stations with large river beds, natural river sections (no weirs) and not many gaugings make the uncertainty on discharge values quite high. In a first attempt to quantify this uncertainty, a methodology called BaRatin (BAyesian RATINg curve) (Le Coz et al., 2014) has been used for the Ardèche catchment (Horner, 2014) providing the most probable stage–discharge relationship and the associated 95% uncertainty. Then, the propagation of all the sources of uncertainty in hydrographs has been done resulting in quantiles 2.5% and 97.5%. The results are still preliminary, but are shown on Fig 1. The figure also shows that the model behaves quite well especially in autumn and spring conditions and that discharge uncertainty is particularly large.

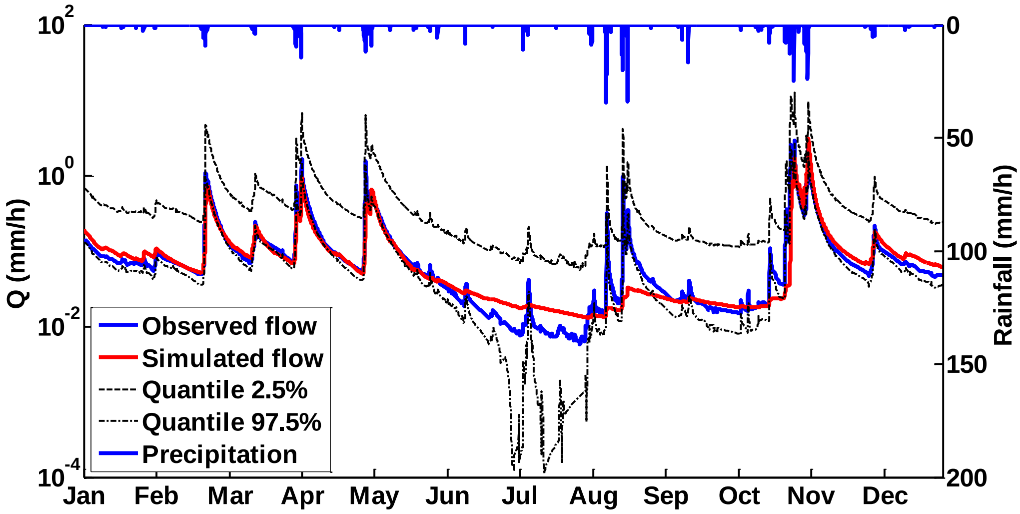

Figure 1. Series of simulated hourly hydrographs (red) for the Ardèche at Meyras (#1) catchment for the year 2004, compared with observed discharge (blue). Dashed lines correspond to the quantiles 2.5% and 97.5 %.

This figure is presented in the on-line answer to Reviewer#2[3], but not in the revised version of the manuscript as these are only preliminary results.

We added some comments about discharge uncertainty in Sect. 5.1 – data quality (p. 26 lines 9-14, and a reference to ongoing work on hydrograph uncertainty Branger et al. (2015).

32-29: How many rain gauges did you use? What was the rain gauge density?

There is only one raingauge located in the Ardèche at Meyras catchment that was used in this study. It can be also seen in Fig. 1 in the article. The Ardèche catchment is quite well covered by operational rainfall networks and research networks (see Figure 2 of Braud et al., 2014). Unfortunately, this was not the case for the four catchments, studied in this paper. There also exists kriged hourly rainfall within the OHM-CV observatory (Boudevillain et al., 2011) but only for selected events. So we had to use the SAFRAN reanalysis at the 8x8 km^2 to get data for all the catchments.

Sources of rainfall data used in the study are presented in Sect. 2.2.1 (p.7 26-33).

33-11: Can you justify the assumption that potential ET is equal to actual ET?

This point was also raised by Reviewer#1 and we provide here the same answer that the one provided for their comment 2). We would like to highlight one point which was probably not fully clear in the paper presentation. In fact we assume that $AET = \alpha_{AET} * K_c ET_0$ where α_{AET} is the scaling *AET* factor provided in Table 3 of the original paper. While this scaling factor is assumed to be constant throughout the year, hourly variation and seasonal variations of *AET* are considered. We agree that a mean annual value of α_{AET} is probably too coarse, because strong seasonal variations in *AET* signal are expected due to the seasonal variations of ET_0 and vegetation activity. However, the Turc (1951) formula only provides annual values of *AET*. The method of Thornthwaite and Mather (1955) cited by Gudulas et al. (2013) provides

[3] http://www.hydrol-earth-syst-sci-discuss.net/11/C6172/2015/hessd-11-C6172-2015-supplement.pdf

monthly estimates of *AET* and could be a way to improve our simulations. Many studies discussed the role of actual evapotranspiration in autumn and winter periods.

For example, Boronina et al. (2005) found that in Mediterranean Cyprus actual evapotranspiration was close to potential rate during the November-March period since there was always water present in the air and soils. In other seasons he argues that evapotranspiration probably occurs from the groundwater table too.

To avoid confusion, the way *AET* is computed in our study has been removed from Sect. 3.1 (g(Q) estimation as it is not used in this section) and is now presented in Sect. 3.2 "discharge simulation". We also better highlight that for catchments #2, #3 and #4, the rescaled $K_C ET0$ was used for the discharge simulation. The relevance of the *AET=PET* hypothesis is also further discussed in Sect. 5.1, p. 27 lines 21-31.

33-28: Does the water content in the air matter for ET reduction?

When detailed models such as Soil Vegetation Atmosphere Transfer (SVAT) models are used to compute actual transpiration or evaporation, vapor pressure deficit is taken into account in the computation. Therefore it has an influence on possible reduction of *AET*. But this cannot be taken into account in the simplified approach proposed in our paper. Only air humidity is taken into account in the computation of reference evapotranspiration ET_0, leading to higher ET_0 if the air is drier.

34-13: Could there be other causes for the non-closure of the water balance? Is it possible that there are other terms that are not accounted for, such as groundwater flow into/out of the catchment, groundwater or surface water abstraction, etc? How certain are you of the catchment sizes you estimated? Over-/underestimation of the catchment size could of course also lead to an under-/overestimation of the specific discharge.

In the studied catchment, there is no regional aquifer. So water balance closure problems due to groundwater boundaries being different from the topographic boundaries are unlikely to occur. Another cause of possible failure of water balance closure is agricultural uptake, which is not known to us for the examined catchments. Regarding the catchment sizes, we think that catchment sizes correspond mostly to the hydrographic network even though the geology is quite heterogeneous.

This was partially introduced in Sect. 2.1 (p.6 13-14).

35-18: When reading this the first time, I was surprised that you doubted the precipitation as well. You explained the problems with ET in detail, but did not mention the problems with P until the explanation in the discussion (starting in 53-23). I may have read over it, but maybe (a summary of) this discussion could be mentioned earlier.

This remark is implemented in the article and mention of possible rainfall underestimation by SAFRAN is included sooner in Sect. 2.2.2 (p.9 16-19).

37-7: Is catchment #1 "accidentally right" or do you have reasons to have more confidence in this catchment than in the other catchments? What do you think causes the mismatch in #2, #3 and #4 and the match in #1?

The catchment #1 is used as a representative catchment in this study mostly due to the good precipitation estimation with local data. The raingauge, as can be seen in Fig.1, is located in the middle of the catchment, and thus probably better captures average whole-catchment precipitation. Other catchments have no local raingauge station within the catchment boundaries (#3) or there was lack of data for certain periods within the years (#2 and #4). Using the SAFRAN reanalysis in these catchments however, water balance has not been closed. This led to the rescaling analysis for catchments #2, #3 and #4.

This is also better explained in Sect. 2.3 and p.11 lines 8-14.

39-9: Being able to estimate g(Q) from Q observations when P and ET are zero is extra advantageous in your case, because the corrections to P and ET are multiplicative, so this analysis does not depend on the rescaling you used.

This is a useful point, although the rescaling does have some slight effect on g(Q). Recession analysis does depend only on discharge time series. However, the selection of points that are retained in the analysis will depend on rainfall thresholds. As rainfall is modified due to rescaling, the points used in the g(Q) estimation are not exactly the same when rescaling is applied, leading to slight differences in the g(Q) parameters. This is commented in Sect. 5.1 and p.25 lines 9-13.

39-23 "we avoided the vegetation period for the estimation of the g(Q) function": Does this introduce a bias towards the peak? Is it reasonable to assume that the behaviour of the catchment in a very wet state is similar to the behaviour in an average or dry state? A short back-of-the-envelop calculation: in Fig. 5, the ln(Q) of the lowest bin you used for the regression analysis is at about -1.5. That amounts to exp(-1.5)=0.22 mm/h. If I then look at Fig. 6 or Fig. 3, that means that only a limited section of the hydrograph is used for the regression analysis. I can imagine that eliminating summer and not using the scatter in the lower discharge regime are necessary for the application of the method, but I think it is important to mention the possible consequences of this decision.

We agree with Referee#2 that the selection of the low-vegetation period as well as the use of thresholds in the data binning implies that only some parts of the hydrographs are sampled. It is likely that the catchment behavior in wet state cannot be really considered as similar to that in the dry state. This can be clearly seen in the not-so-good modeling performance in the summer period. There, the model does not succeed to reproduce hydrographs probably due to the lack of wet antecedent moisture conditions which are usually present in low-vegetation periods. In addition, as mentioned in the paper (p.46 26-27 and p.47 1-4), we tried to estimate the g(Q) function for vegetation and low-vegetation periods, as well as by considering all the data (see details in Adamovic, 2014), and the C_3 coefficient was generally positive, which certainly shows that results are influenced by evapotranspiration. Wittenberg and Sivapalan (1999) applied a stratified recession analyses, depending on the vegetation state, to catchments in Australia and proposed a method to also retrieve evapotranspiration. Their approach can be used to improve our analysis. This reference is added in the revised version of the paper in Sect. 1 (p.3 31-32).

40-17: Why did you chose a quadratic function? Based on Fig. 5 I would choose a linear relation, eliminating already one parameter.

We fitted also linear relation to the binned means. However, quadratic parameter C_3 was statistically significant ($p<0.1$) and thus a quadratic function is kept as a representative function for fitting in the article. Melsen et al. (2014) concluded in their work that a two-parameter model is reasonable able to capture high flows but they fail to describe the low flows. In our analysis we therefore used the three-parameter model where the third parameter C_3 is essentially related to the low flows in order to capture the catchment behavior in that flow regime.

We also introduced this reference in Sect. 4.1 (p.19 17-20).

41-11: When I read this the first time, I wondered how you determined the ranges. Of course, 10,000 can be a small number when the parameter range you choose is very large. You do mention how you got to these ranges later, but I think it's good to mention it shortly in Sec. 3.5 as well.

We agree with Referee#2, and this is modified in the manuscript accordingly in Sect. 3.5 (p.18 7-8).

46-15: The scatter is large in log space, but small in linear space. Very small fluctuations at low discharges, caused by small variations in the storage-discharge relation (hysteresis?), may appear more substantial than they really are.

We agree with Referee#2. Apart from already mentioned reasons in the paper (46-15), the small variations could also come from the significant discharge diurnal cycle that is mentioned in the paper (40 1-3). These variations could be due to the diurnal cycle of transpiration of riparian vegetation for example. We also recognize the fact that the log space gives more weight to small variations which are not really important for "real life" applications.

47-4: Just out of curiosity: I had the problem that in dry periods, the modeled storage volume was very small and Q+ET exceeded S. I had to limit Q and ET to avoid negative storage. Did that happen in your catchment as well?

In the SDSA, storage measures are relative rather than absolute (as Kirchner's original paper makes clear in several places). One always works with derivatives of storage (and if one did integrate them, one should remember that the constant of integration can have any value). Thus we are puzzled by the mention of "negative storage".

49-24/26: If you plot the curves with the altered parameters in a (Q, -dQ/dt)-plot, do you see that changing the value of c3 leads to similar values of –dQ/dt at high –Q, but different values of –dQ/dt at low Q? The location of the line could explain why you only see a difference during low flow periods.

Referee#2 is right. The location of the line explains why there is only a difference during low flows.

51-7 "not overparameterized": I'm not sure I agree completely. I suspect that the parameters are highly dependent. Did you plot response surfaces of the outcomes of the Monte Carlo simulation to investigate this? I don't think you have to show it in your paper, but you may want to inform the reader of the outcomes.

We have changed the statement. The point is that not that the parameters are independent, but that they are well constrained by the recession plots. It is also encouraging that the parameters from the recession plots agree with the "behavioral" parameter ranges. We also agree that the way we have done our curve fitting, the three parameter values will depend on each other. One can make the curve fitting parameters nearly independent by "centering" the ln(Q) variable (many polynomial fitting packages now do this automatically).

This is modified in the revised article in Sect. 4.4.2 (p.23 6-9).

52-12 "representative of Mediterranean catchments": Is this really true? I would expect that the Ardèche is much wetter than the average Mediterranean catchment. And, as you see, the drier the catchment becomes, the more difficult it is to apply the sdsa.

Referee#2 is right. We propose to modify the text as follows "… *the Ardèche catchment representative of Western Mediterranean catchments*". Those catchments have intense rainfall events in autumn and dry summers. Moreover, the Ardèche has also a mountain influence with snow in winter, which makes it not fully representative of Mediterranean catchments.

This is modified in the revised version of article, in Sect. 5 (p.24 7) and in the conclusion, p. 30.

We also agree with the Referee#2 that the drier the catchment becomes, the more difficult is to apply the sdsa as in summer periods in the Ardeche.

52-19 "more arid": Maybe change this to "less humid". Annual rainfall of 1400 to 2100 mm is far from arid in my opinion.

We agree with the Referee#2. This is implemented in the revised article in Sect. 5.1 (p.24 13)

56-2 "however": I think this word should be left out, as your conclusions are not in contrast with mine. In fact, they point in the same direction: the sdsa works when it's wet enough. The Hupsel Brook catchment receives about 800 mm rainfall annually and the runoff ratio is much lower than in the Ardèche.

We agree with the Referee#2. This is implemented in the revised article (Sect. 5.1, p.28 3-4).

Table 3: In 53-25 you say that "SAFRAN is known to underestimate precipitation". Why did P at catchment #3 decrease after correction? Can you justify this correction?

SAFRAN is known to generally underestimate precipitation in mountainous regions. However, SAFRAN rainfall is estimated based on so-called "*symposium regions*" which are assumed to be climatologically homogeneous. And rainfall from one symposium to the other can be quite different. If those regions are not well delineated, this may lead to incorrect estimation of rainfall amounts, including rainfall underestimation. Moreover, catchment #3 is particularly small, so very local factors are more likely to be more important.

This is modified in the revised version of article, in Sect. 5.1 (p.25 21-24).

Table 6: It is striking that the year-to-year variation in NSE is very large, with some very good results. For operational purposes, this can be a challenge. After a year with good results, people can come to trust the model, which then fails completely the next year.

We agree with the Referee#2. This is also mentioned in the revised article in Sect. 4.2 (p.20 10-12).

Figure 6: I am surprised that the peaks in August are underestimated, even though the discharge (and therefore storage) in July is overestimated. Can you offer an explanation? Does this happen in more of your runs? And if so, what does this mean for practical applications? I can imagine that for water managers this is the most important peak of the year to simulate well.

We thank Referee#2 for this interesting remark. Discharge underestimation in August and overestimation in July is seen in other years too. After the May rainfall event the system is emptying slowly leading to the discharge overestimation in July and thus later discharge underestimation probably due to the lack of antecedent moisture in August. May be there is a bypass flow phenomenon which is not captured by the model. This also points out that the model is not so recommendable at current state to be used by water managers for the summer period. However, the model has perspectives to be used for flash floods.

Figure 7: In catchments #1, #2 and #3 the inferred precipitation is often high (up to 250 mm when the "observed" is zero. Do you know when this occurs? When Q is small and a small fluctuation in Q has a large effect on the modeled storage? And why does it not happen at #4?

Referee#2 is correct. Precipitation overestimation in vegetation periods can happen when Q is small and a small fluctuation in Q (possibly due to measurement error!) has a large effect on the modeled storage. This happened in catchments #1, #2 and #3. We think that this is not the case for catchment #4 where discharge fluctuations in summer periods are probably not so frequent since there have been not so many precipitation events as it can be seen in Fig. 6.

You often refer to the sdsa as "the Kirchner method" (29-28, 32-2, 32-635-14, 35-17,37-25, 35-14, 35-17, 37-25, 48-9, 52-2, 52-11, 55-21, 55-36, 56-10, 57-20 - I may have missed some).

As James Kirchner is one of the authors, I think it would be more appropriate to call the method "simple dynamical systems approach".

We agree with Referee#2. This has been implemented throughout the article.

Technical corrections

Some sections contain many short paragraphs of only a few sentences. Perhaps sometimes paragraphs could be joined to keep the "flow" of the reader (e.g. 39-11, 38-15, 39-25). But this is a matter of taste of course.

31-4: Historical data have shown > Historical data show?
Corrected in the article (Sect. 2.1, p.6 4).

32-3: "discharge data. The latter" >"discharge data, which"
This sentence was suppressed and the following sentences were modified in Sect. 2.2.1 (p.7 3-11)

33-7: double bracket after "(#3)"
Corrected in the article.

33-8: "we also use">"we also used"
Corrected in the article (Sect. 2.2.1, p.8 1-2).

33-10: "using Penman-Monteith formula">"using the Penman-Monteith formula"
Corrected in the article (Sect. 2.2.1, p.8 3).

37-26: "in-consistency" >"inconsistency"
Corrected in the article (Sect. 3, p.12 2).

38-23: Is "differentiating" the appropriate term here?
Corrected in the article as : "*dQ/dS can also be expressed as a function of Q, following Kirchner (2009) as:*". (Sect. 3.1, p.12 23)

42-9 run>ran
Corrected in the article (Sect. 3.2, p.15, 28).

42-13: "spatial-temporal">"spatial". You don't have to mention temporal variation, because the point you want to make concerns spatial variation only.
Corrected in the article (Sect. 3.3, p.16 3).

47-28 "explanation to" >"explanation of".
Corrected in the article (Sect. 4.2, p.20 15).

55-14: "9 year">"9-year".
Corrected in the article (Sect. 5.1, p.27 13).

Table 2: You can remove "Crop coefficient" from the Table, as it's already in the caption. You could add the periods (e.g. "Jan.-May") belonging to each period.

Corrected.

Catchment name	K_c_initial (Jan.-Apr.)	K_c_mid_season (May-Oct.)	K_c_late_season (Nov.-Dec.)
The Ardèche at Meyras (#1)	0.74	0.94	0.79
Borne at Nicolaud Bridge (#2)	0.73	0.96	0.80
Thines at Gournier Bridge (#3)	0.68	0.94	0.75
Altier at Goulette (#4)	0.62	0.97	0.75

Table 2. Weighted average crop coefficient for each examined catchment per growing stage

Table 3: This Table is somewhat confusing. Maybe it would help to make the numbers for your final estimate of P, ET and Q (the ones you actually used in the model) bold.

Both referees highlighted that Table 3 should be clearer. Therefore, we divided this table in two parts, one table providing the information about main terms of water balance equation (P, Q, C, AET, ET_0, K_cET_0) and the second table that gives details about coefficients when using flux scaling and corresponding variables (AET_{Turc}, T, P_{Turc}, C_{Turc}, C_n). They are also better introduced in the manuscript as proposed by Referee#1.

Table 5: You can remove "Non-vegetation period" as it is already specified in the caption.
Corrected.

Catchment name (ID)	C_1	C_2	C_3
The Ardèche at Meyras (#1)	-3.74	0.65	-0.2
Borne at Nicolaud Bridge (#2)	-4.08	0.74	-0.15
Thines at Gournier Bridge (#3)	-3.71	0.72	-0.13
Altier at Goulette (#4)	-3.80	0.82	-0.02

Table 5. *Parameter values for the examined catchments for all non-vegetation periods (2000-2008).*

Table 9: You can remove "Lower/upper bound" as it is already specified in the caption.
Corrected.

Parameters	C_1	C_2	C_3
Parameter range	[-1] – [-6]	[0.1 –1]	[-0.001]– [-0.5]
The range of "behavioral" values	[-3.5] – [-4.5]	[0.1 –0.9]	[-0.001]– [-0.25]
Reference (from recession plots)	-3.74	0.65	-0.2

Table 9. Comparison of the chosen parameter range and parameters obtained from non-vegetation periods for *the Ardèche at Meyras (#1) catchment.*

Table 10: You can simplify this Table by moving "SAFRAN rain" to the caption, remove the words "Catchment" and move the catchment names to a column in front of the performance measures.

We simplified the Table 10 as suggested by the Referee#2.

Catchment	**Performance**	**Operational**	**Rescaled *P***	**Rescaled *P* and *AET***
Borne at Nicolaud Bridge (#2)	*NASH*	0.45	0.65	0.67
	NASH log	0.58	0.70	0.61
	PBIAS	42	14.2	0.75
Thines at Gournier Bridge (#3)	*NASH*	0.36	0.50	0.55
	NASH log	0.79	0.62	0.78
	PBIAS	-13.8	22	0.98
Altier at Goulette (#4)	*NASH*	0.54	0.79	0.74
	NASH log	-4.90	-2.99	0.18
	PBIAS	49	23.65	-0.29

Table 10. Model performance for three examined catchments over the whole examined period (2000–2008); Comparing the original operational data and rescaled precipitation and evapotranspiration data.

Figure 3: "julian days">"Julian days". I would actually prefer months on the x-axes, because you don't use Julian days in the rest of the paper. The Q, ET and P are missing on the y-axes. You don't need to show the x-axis for the top two plots as they are the same as in the bottom plot. Could you indicate the non-vegetated period in this Figure?

We agree with the Referre#2 that in Fig. 3 rather months than Julian days should be presented since we used non-vegetation periods in our analysis. Therefore we also colored non-vegetation period in blue as suggested by Referee #2.

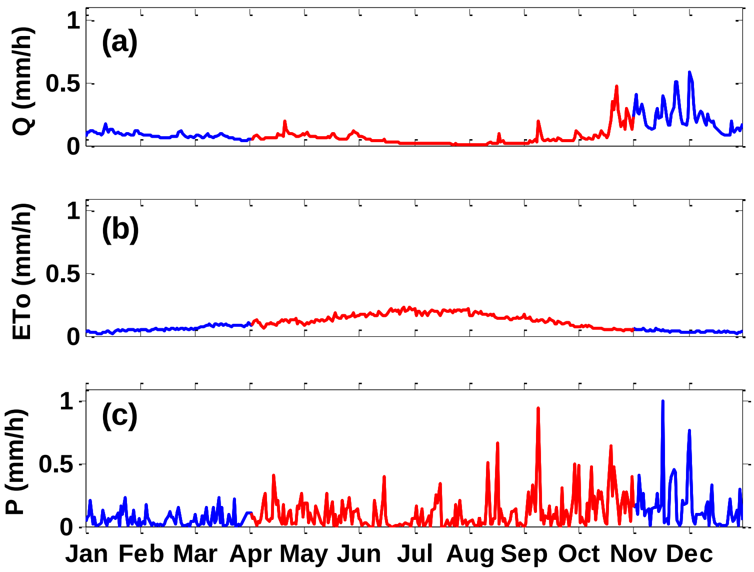

Figure 3. Average hourly discharge (a), reference ET_0 (b) and rainfall (c) in [mm/h] at the Ardèche outlet for all months between 2000–2008. (b) and (c) are calculated from the SAFRAN reanalysis. In red: vegetation period; in blue: non-vegetation period.

Figure 5: I prefer log axes to plotting ln(Q), like you did in the other Figures, so it's easier to compare to the hydrographs (for example to see which part of the range of Q is used). You also used mm/hr instead of mm/h.

We agree with Referee#2 that it would be also useful to plot the Fig. 5 on log axes. We did this for the plot on the left whereas for the plot on the right we kept the ln(Q). We have also corrected the units (mm/h).

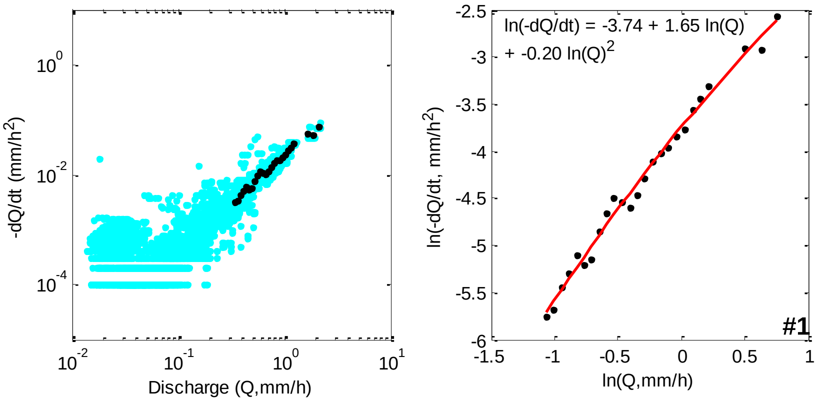

Figure 5.Recession plots for the Ardèche at Meyras (#1) catchment for all non-vegetation periods between 2000 and 2008; (left) Flow recession rates (−dQ/dt) as a function of flow (Q) for individual rainless night hours (blue dots) and their binned averages (black dots). (right) Quadratic curve fitting with binned means.

Figure 6: The right y-axis label "P (mm/h)" is missing. I would prefer a second panel with the discharge shown in linear space to see how good or bad the fit in June and July is for "real life" applications.

The Fig. 6 has been also modified. As a supplement to this figure we plot another figure here in linear space to show the fit in summer periods for "real life" applications.

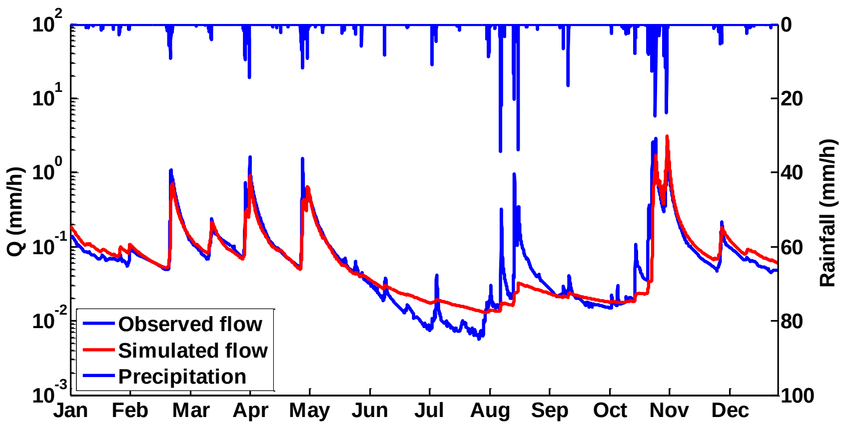

Figure 6. Series of simulated hourly hydrographs (red) for the Ardèche at Meyras (#1) catchment for the year 2004, compared with observed discharge (blue).

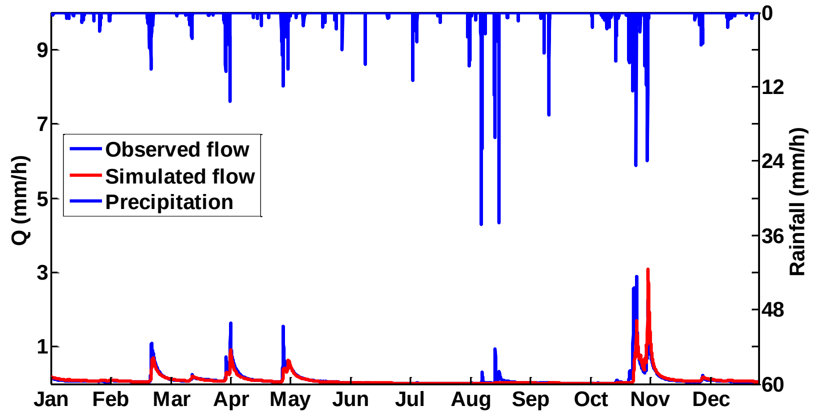

Figure 6-1. Series of simulated hourly hydrographs (red) for the Ardèche at Meyras (#1) catchment for the year 2004, compared with observed discharge (blue) in linear space.

Short comments by students

We thank all the students for their positive appraisal of the paper content and for their constructive suggestions to improve the paper.

In our detailed reply[4] we give answers to all students reviews, taking into account the major issues they mentioned.

All the minor and specific comments are also taken into account in the revised manuscript.

As a general comment, we would also like to underline that, after submission of our paper, we were aware of papers by Wittenberg (1999) and Wittenberg and Sivapalan (1999) where recession analyses were applied to estimate groundwater recharge in an Australian catchment with a Mediterranean type climate. Therefore our study is not the first application in this kind of environment. Wittenberg and Sivapalan (1999) pointed out the impact of evapotranspiration on recession estimation. They also showed, that using a stratification of the data set according to the time in the year, it was possible to quantify evapotranspiration losses and groundwater recharge. We refer to those papers in the revised version of our manuscript where lines 20-21 p10728 are modified as follows:

"To our knowledge, the simple dynamical system approach has not been evaluated in a Mediterranean context, where the rainfall regime exhibits strong contrasts between dry conditions in summer and intense rainfall events, often related to stationary Mesoscale Convective Systems (Hernández et al., 1998), during autumns. Wittenberg and Sivapalan (1999), for instance, used recession analyses to estimate groundwater recharge in a

[4] http://www.hydrol-earth-syst-sci-discuss.net/11/C6174/2015/hessd-11-C6174-2015.pdf

Mediterranean type of climate in Australia, but they did not consider the storage-discharge relationship in its implicit differential form, the sensitivity function g(Q)".

As a second general comment, we would like to apologize about an error in the reference to the Turc equation. The correct reference is Turc (1951) and it has been corrected in the revised version of the paper. In this paper Turc presents the formula for *AET* estimation based on annual average temperature and rainfall, whereas the Turc (1961) papers presents a formula to compute potential evapotranspiration depending on temperature only. This may have led to some confusion, as some students pointed out papers comparing various formula of reference evapotranspiration ET_0, and not actual evapotranspiration *AET*.

In the 1951 paper, Turc reports an evaluation of his formula by comparing measured interannual discharge to values estimated through *P-AET* where *AET* is estimated by formula (2) of the paper with generally good performance. The considered data set was covering countries all over the world. In addition, as described in the paper, one of the reasons for choosing Turc's simple equation for the estimation of *AET* from *P* and *T* in Mediterranean climatic conditions is that it relies only on the *P* and *T* and not on ET_0, we could avoid the use of evapotranspiration and reduce uncertainty in estimating *AET*. In addition, the Turc equation for estimation of *AET* is widely used in France and thus our results can be compared to other studies we can find other studies for comparison.

We modified substantially Sect. 2.3 to include those elements and better introduce Turc (1951) formula, see p. 10 lines 21-27, p.11 lines 8-14.

Concerning *AET* estimation in our modeling, we would like to highlight one point which was probably not fully clear in the paper presentation. In fact we assume that $AET = \alpha_{AET} * K_c ET_0$ where α_{AET} is the scaling *AET* factor provided in Table 3 of the original paper. While this scaling factor is assumed to be constant throughout the year, hourly variation (hourly ET_0 signal) and seasonal variations (seasonal K_c) of *AET* are considered. We agree that a mean annual value of α_{AET} is probably too coarse as strong seasonal variations in *AET* signal are expected due to the seasonal variations of ET_0 and vegetation activity. However, the Turc (1951) formula only provides annual values of *AET* and the water balance approach (*AET=P-Q*) that we used as reference is also valid only for interannual averages. The method of Thornwaite and Mather (1955) cited by Gudulas et al. (2013) provides monthly estimates of *AET* and could be a way to improve our future simulations.
To avoid confusion, the way *AET* is computed in our study has been removed from Sect. 3.1 (g(Q) estimation as it is not used in this section) and is now presented in Sect. 3.2 "discharge simulation". We also better highlight that for catchments #2, #3 and #4, the rescaled $K_C ET0$ was used for the discharge simulation. The relevance of the *AET=PET* hypothesis is also further discussed in Sect. 5.1, p. 27 lines 21-31.

Short comment by Eric Gaume

General comments

First of all, my deepest apologies to the authors and the editors for this extremely late review. The manuscript is interesting, clearly written and documented and in the scope of

HESS, but has in its present form some defaults that have to be corrected before publication. It evaluates the performances of a simple conceptual global rainfall- runoff model based on a 3-parameter non-linear reservoir (eq. 12 of the manuscript) in simulating hourly discharge series of small watersheds. This model and its calibration procedure were initially introduced by Kirchner (2009) and used in several recent works (Krier et al., 2012, Brauer et al., 2013). The application of this approach to Mediterranean watersheds is the main originality of the manuscript according to its authors.

We thank Eric Gaume for his short comment on the paper content. We take it as an opportunity to make a few things clearer and to improve our paper. E. Gaume raises many issues in this comment and we tried to answer them as precisely as we could.

First, the selected database appears to be of poor quality: the available measured series are short - less than 10 years - and the yearly water balances appear implausible for 3 out of the 4 considered test watersheds, indicating flux estimation errors.
These problems are acknowledged by the authors (p 10734) but their answers are moderately convincing. The authors suggest a correction of both - estimated actual evapo-transpiration and precipitation - to reach an annual balance. As a result, they work on artificial "scaled" data which limits their demonstration. A more in-depth critical analysis of their data would certainly have revealed estimation problems due to poor rating curves (according to published data, the streamflow of the Borne at Saint-Laurent-les-Bains (95 km^2) is equal to 880 mm/year, comparable to the other provided data). Likewise, the precipitation amount on the Altier Watershed (4) is surprisingly low if compared to the other available values. The whole work would have been much more convincing if based on good quality data.

We agree with the Reviewer that data quality issues are important. This aspect was also pointed out by the students' reviews and some elements can be found in the reply to their comments[5]. Several answers can be given:

- First, we agree on the interest of well monitored and controlled catchment for scientific studies. However, here we were specifically interested in testing the simple dynamical system approach for catchments, where only operational data are available, and that are more representative of the real world. This specific objective was also highlighted in the paper, but maybe not clearly enough.

- We chose to focus on the Ardèche catchment in this study because we wanted to be able to document site-specific conditions according to local knowledge, which was made possible in the framework of the Floodscale project (Braud et al., 2014). As a preliminary step to this study, we thoroughly analyzed the stations and their functioning with the help of the local authorities in charge of the network (SPC Grand Delta and EDF) who provided the rating curves and gaugings. The stations that are influenced by dam operations (Ardèche at Pont d'Ucel, Pont de Labeaume, Vogüé, Vallon Pont d'Arc, Sauze, Chassezac at Gravières) or present obvious rating curve or continuity problems (Beaume at Rosières, Volane at Vals-les-Bains) were discarded from our dataset. This explains why we ended up with only 4 stations. As indicated by the Reviewer, another strategy would have been to use research-grade data from experimental watersheds, but it is not the purpose of such a data-driven approach,

[5] http://www.hydrol-earth-syst-sci-discuss.net/11/C6174/2015/hessd-11-C6174-2015.pdf

which loses much of its interest if the contact with local knowledge is lost (see below for further development on this specific question). However, we would like to underline that the work of a Master student recently applied the same approach to a larger sample of catchments (20) in the Cévennes area (Coussot, 2015; see also reply to Referee#1[6]). Although preliminary, the results of this Master work confirm our results, with the same issues of data quality and catchment mass balance, and same conclusions on the applicability of the simple dynamical system approach on Mediterranean catchments. In conclusion, this study enabled us to identify the problems with measurement networks, on the one hand, and to better understand the catchment functioning taking many climate forcing uncertainties into account, on the other hand. We believe that our results are of interest, as they point out that, when provided data uncertainty is correctly handled, the simple dynamical system approach is applicable to Mediterranean type catchments.

We consider that using operational data is an originality of the paper, as also pointed out by Reviewer#2. We better highlighted this point in the introduction (p.3 24-26), in Sect. 2.2.1 (p.6 29-30). In the discussion, we also mention that the work presented in this paper has been extended to 20 catchments of the Cévennes-Vivarais region, highlighting the same kind of water balance closure problem as in the present study (see p. 26, lines 5-8).

- On our sample of stations, it seems that discharge estimation problems due to poor rating curves are not the main problem. Ongoing work focuses on the estimation of rating curves and their uncertainties (Le Coz et al., 2014; Branger et al., in preparation, see also Reply to Referee#2[7].). The application to stations in the Ardèche catchment shows that the uncertainty related to rating curves, although not negligible, especially for peak flow values, is not an explanation for the mass balance discrepancies that were found in the data. The main problem comes from the estimation of the precipitation and/or evapotranspiration. Evapotranspiration is not measured, and precipitation is difficult to measure in these mountainous areas. In the particular area of the Altier catchment (pointed out by the Reviewer), we used the SAFRAN reanalysis which was the only continuous rainfall data source for the upstream areas. SAFRAN has drawbacks: in particular it seems to underestimate the rainfall in the particular area of the Altier catchment. Other rainfall estimations are being developed in the framework of the Floodscale project (see papers by Delrieu et al., 2014), but were not yet available at the time of our study.

We also extended the discussion about discharge data quality in Sect. 5.1 (p.26 9-14) as well as rainfall data p.25 lines 21-24.

- We must also point out that the values given by the Reviewer for the Borne at St Laurent les Bains – Pont de Nicoulaud catchment are erroneous, whereas the values in the paper are correct: the catchment surface is ~63 km² and not 95. We calculated the catchment surface based on the position of the station and the 25 m IGN DTM. The Banque Hydro database is in agreement (62.7 km²). However, the Wikipedia page of the Borne river[8] provides this 95 km² value, which may come from a previous erroneous publication. We will suggest a correction. Thus, a streamflow of 880 mm/year is also erroneous; the value presented in the paper (1579 mm/year) is correct considering the 2000-2008 period (the Banque Hydro database indicates 1357 mm/year for the 1969-2011 period).

[6] http://www.hydrol-earth-syst-sci-discuss.net/11/C6170/2015/hessd-11-C6170-2015.pdf

[7] http://www.hydrol-earth-syst-sci-discuss.net/11/C6172/2015/hessd-11-C6172-2015.pdf

[8] http://fr.wikipedia.org/wiki/Borne_%28rivi%C3%A8re_de_l%27Ard%C3%A8che%29

- The purpose of the rescaling of rainfall and evapotranspiration input data is precisely to take into account these inconsistencies in the dataset that could not be solved using only available measurements. This explicit operation avoids, for example, having the model parameters compensate for the input data uncertainty, which is a common problem of conceptual hydrological models. It also allows for a more objective evaluation of the model performance, because a model that is based on mass conservation cannot work successfully on catchments with obvious mass balance problems.

We added comments on this point in the discussion (Sect. 5.1 p.25 9-16).

Moreover, the lengths of the available series does not allow for a validation of the calibrated models. To my opinion, validation (based on split-sample tests) is an absolutely necessary step of any model implementation work in hydrology. No work should be published without validation results. This is missing here and should absolutely be added.

We do not agree with this remark. For example in the work of Melsen et al. (2014), the authors concluded that one winter season (November until March) can give reasonable results with two-parameter model in a small Alpine catchment (3.31 km^2). Considering that the Ardèche catchments are larger and more heterogeneous in terms of geology and land-use, we considered a nine year period which is sufficient to estimate the parameters of the g(Q) function in a robust manner.

We added sentences to Sect. 3.1 to better explain this point (p.13 14-19).

The Reviewer also points out that the results are not validated using independent data. We cannot agree with that either. The g(Q) function is estimated using only a small part of the streamflow time series: only nighttime, rainless hours during the non-vegetation periods (November-March) of each year. The model is then run without additional calibration for the rest of the year. Therefore, although not a classical split-sample test, the model validation is performed on independent data. This point was already discussed in the first version of the paper in Sect. 5.1 (now p.27 11-16).

Second, the authors put forward the novelty of the proposed approach. This is also questionable. This approach is not uninteresting in its formulation, but far from new. What is proposed is a relatively standard method based on a non-linear reservoir for simulating recession curves. Such models exist since the very first hydrological model development works in the late sixties. The 3-parmeter non-linear reservoir drainage law (eq. 12) may be new. But by the way, the justification for the specific form of equation 12 is missing. Even, the retrieval of rainfall based on discharge measurement is not new: it was for instance the objective of the so-called DPFT method developed in France and that authors certainly know and should have cited (see for instance Sempere Torres et al, Natural Hazards, 1992). Finally, the proposed approach leads to the development of a 4-parameter conceptual rainfall-runoff model (3-parameters for the non-linear reservoir and 1 parameter for the rescaling of data ensuring mass- conservation), and this model only works in winter times. This is not particularly novel. Many conceptual models have been proposed and tested during the last 30 to 40 years in hydrology and it would be essential to evaluate the added value of the *proposed model, comparing it to other existing models of the same type. This comparison should be added to my opinion in the proposed manuscript.*

We agree with the Reviewer that many recession models date back to the late sixties. However, there must be misunderstanding in the specific originality of the simple dynamical system approach. What is new in this approach is not the reservoir itself, but the manner to derive its structure and parameters from the data analysis: in particular, here the functional form of the storage-discharge relationship is not specified a priori, but determined directly from data (Kirchner, 2009). This is the very definition of the top-down or data-driven modelling approach, that is acknowledged to be a major paradigm shift in modelling by the hydrological community that occurred during the PUB decade (see for instance Sivapalan, 2003; Hrachowitz et al., 2013). Therefore we argue that testing this kind of approach on new datasets, for various climatic conditions, contributes to the advance of hydrological science in itself. We have also compared the model results with other models that are based on similar data-driven methodology (e.g. Brauer et al. (2013) and Melsen et al. (2014)) and obtained similar results. This mention was maybe not clear enough and will be added in the paper. The comparison with other more parametric models is not relevant for our study.
We better highlighted the interest of testing the sdsa approach in the Mediterranean context in the introduction, p.3 lines 24-30.

A few more detailed remarks:

- We are aware of the DPFT method proposed by Torres et al. (1992) and further revisited by Duband et al. (1993). However, the purpose and principles of the DPFT are different from the approach presented in our paper. The DPFT method is an event-based method where net precipitation and the unit hydrograph are identified at the same time, using optimization techniques between the simulated and observed discharge. In our application, the discharge sensitivity function g(Q) is estimated using non-vegetation periods (not only for selected events), and is derived from data analysis only (there is no optimization between measured and simulated discharge, which would make reproducing the hydrograph into a nearly trivial exercise). In our paper, the discharge simulation and the rainfall retrieval are two ways of assessing the relevance of the identified discharge sensitivity functions, which is estimated a priori, using only discharge fluctuations and discharge data (rainfall is only used for the selection of the points used in the g(Q) estimation).

- The SDSA model consists of 3 parameters and not 4. The rescaling is not systematic and performed independently from the model performance as explained before. It is just a way of ensuring mass balance in the catchment so that the model does not have to compensate for problems in the input data. Thus it cannot be considered as a calibration parameter (by the way, there is one rescaling coefficient for precipitation and one for evapotranspiration, which makes 2). In our study, no rescaling was done for the Ardèche at Meyras catchment. The SDSA was also used as a basis for the semi-distributed SIMPLEFLOOD model (Adamovic, 2014), and was applied to the whole Ardèche catchment without data rescaling, based on the SAFRAN forcing. The results pointed out systematic volume underestimation by the model. Further work will use improved rainfall forcing such as the radar/rain gauges reanalyses proposed by Delrieu et al. (2014) to see if those underestimation problems are linked to poor rainfall forcing.

- The judgment that the model works only in winter time is not correct. Our analysis showed that the model performs better during winter periods. However, NSE values calculated on the log of discharge are above 0.70 for 3 of the 4 catchments and the whole 2000-2008 period, which do not indicate that the model "does not work". Maybe the graphs presented in log scale were confusing on that point.

To provide a general answer to this comment, we added a sub-section to Sect. 5.1 of the discussion "interest of the SDSA approach as compared to other hydrological modelling approaches" (see p.28, lines 8-23).

Finally, and line with this last comment, the whole manuscript gives the uncomfortable impression that the authors try to reinvent hydrology and hydrological modelling from scratch, without considering the past. One of the last comments of the paper on page 10756 is particularly illustrative of this state of mind. "Our result suggest the existence of another storage, probably more superficial than the "Kirchner" storage which could be used to supply evapotranspiration...". What a discovery ! This reservoir is called soil and taken into account in most of the RR models and the central concern of the SWAT models. This certainly false impression could easily corrected by a better formulation and putting less emphasis on the novelty of the proposed method.

We are not sure to understand this remark very well. The data-driven approach that was proposed by Kirchner and was tested in this study is not just reinventing the wheel; it presents real advantages in terms of consistency between model structure, parameters and observed data, as already explained above, and it is amenable to much more rigorous testing than typical RR models, since one never optimizes any time-series fits to either precipitation or runoff. The models obtained through this approach are simple, with a limited number of parameters that can be estimated from the available data. The main hypothesis underlying the SDSA approach is that the major contribution to the flow can be approximated by drainage from a single non-linear reservoir, the form and parameters of which can be estimated directly from recession analysis.

Our analysis shows that for our catchments that have high evapotranspiration rates in summer, the simple assumption that was used so far (*PET=AET*) is not fully adequate, and that an alternative evapotranspiration model should be used. Adding a superficial storage to the existing one is one possible way of dealing with this. This could be seen as a superficial soil reservoir.

However, we interpret that the subsurface flow that is produced by the current model comes probably also from the soil. We don't think that attributing specifically a specific originating zone for each flow component is of particular interest for a top down approach.

Finally our results still confirm that the main mechanism we are speaking about is quick sub-surface flow which transits through the reservoir considered in the Simple Dynamical System Approach.

To better discuss the interest of the SDSA approach in terms of catchment functioning, we enhanced Sect. 5.2 of the discussion, by adding references to previous work and identified dominant processes in the region (see p. 29, lines 7-15 and 18-31).

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