Hydrol. Earth Syst. Sci. Discuss., 11, C6138–C6141, 2015
www.hydrol-earth-syst-sci-discuss.net/11/C6138/2015/

Hydrology and Earth System Sciences
Discussions
Open Access

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.

E. Gaume

eric.gaume@ifsttar.fr

Received and published: 19 January 2015

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.

Despite its quality (clear and comprehensive), I am nevertheless hardly convinced by the content of the submitted paper for several reasons related to the approach itself and its implementation.

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 km2) 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. 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.

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

HESSD
11, C6138–C6141, 2015

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.

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.

As a conclusion, the proposed work does not appear as novel to me as the authors suggest, but still could be of interest for the readers of HESS. I would suggest a possible publication with major revisions. The revised version should contain two essential ingredients: 1) model validation results (richer datasets could be therefore used and they exist), 2) comparison with other existing conceptual rainfall-runoff models.

HESSD

11, C6138–C6141, 2015

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 10725, 2014.

HESSD

11, C6138–C6141, 2015