DOES THE SIMPLE DYNAMICAL SYSTEMS APPROACH PROVIDE USEFUL INFORMATION ABOUT CATCHMENT HYDROLOGICAL FUNCTIONING IN A MEDITERRANEAN CONTEXT? APPLICATION TO THE ARDÈCHE CATCHMENT (FRANCE)

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OVERVIEW

SUMMARY

This paper studies whether the simple dynamical systems approach can provide useful information about catchment hydrological functioning in a Mediterranean context (the Ardèche catchment). Since catchments show a high degree of heterogeneity and variability, questions are raised about the degree of complexity that must be used to model their behavior. Recently, Kirchner showed that a simple model can suffice, in which discharge is only based on the amount of storage in the system. However, this method has not yet been evaluated in a Mediterranean context, making this study a potential benchmark for application of the method in Mediterranean catchments.

Adamovic et al. concludes that the Kirchner approach works best in wet periods with low evapotranspiration. In periods with high evapotranspiration, this approach is not able to correctly predict the discharges. While these findings are in line with other studies, the manuscript nonetheless makes a significant contribution. I found three points which will improve the performance of the model (the used SAFRAN data, the Turc equation and surface runoff). By including surface runoff, a review on whether the Turc equation gives satisfying results (with or without a correction on the temperature) and by presenting the known SAFRAN errors, the quality of the research will increase.

RELEVANCE

The methodology described by Kirchner (2009) has already been tested for several other catchments (for instance catchments in Plynlimon, Wales (Kirchner, 2009); Rietholzbach, Switzerland (Teuling et al., 2010); Hupsel Brook, Netherlands (Brauer et al., 2013)). Every tested catchment had roughly the same climate (high precipitation, with generally low evapotranspiration). In this paper, the methodology is tested for a different catchment, which is situated in the Mediterranean area. This area is generally drier, with higher evapotranspiration and seasonal precipitation. Kirchner's method has not yet been applied to a catchment with this climate. When this approach performs satisfactory, it provides an excellent base for future model studies, since the model can easily be applied to other Mediterranean areas.

MAJOR COMMENTS

I found three points where the quality of the research could be improved. These points are all linked to previous researches. Each point is described in a subsection. My recommendations to improve these points are described at the end of each subsection.

ERRORS IN THE SAFRAN DATA

The first point affects the SAFRAN data. In section 2.2.1 the authors describe that they will use the SAFRAN reanalysis of Météo-France for sub-catchment 2, 3 and 4. As mentioned in the discussion (in section 5.1.2), the SAFRAN reanalysis is known to have issues. Vidal et al. (2010) and Quintata-Seguí et al. (2008) already studied the performance of the SAFRAN reanalysis for the entire France. Quintata-Seguí et al. (2008) showed the BIAS and the RSME for every variable (including precipitation), for the entire France. Graphs in this paper show the biggest BIAS and RSME in the mountainous areas, thus including the Ardèche catchment.

Knowing these errors beforehand, I am surprised they were mentioned only in the discussion. I would like to see this information earlier in the paper. Since the SAFRAN data still is reliable, I agree with the use of it. However, I would like to see that the known errors are taken into account when using the SAFRAN data for the analysis.

USING TURC EVAPOTRANSPIRATION TO RESCALE THE WATER FLUXES

In section 2.3 the process of rescaling the water balance fluxes is described. The actual evapotranspiration is used to give representative precipitation and discharge data for each sub-catchment. The authors chose the method proposed by Turc (1961) to determine the actual evapotranspiration on a yearly timescale. The reason to use this method, above the three other presented methods, is because the Turc equation only depends on precipitation and on temperature. How well each method performs is not taken into account.

Previous research (Gudulas et al., 2013; Kisi, 2013) compared different methods to estimate the actual evapotranspiration. However, there has not yet been a comparison between the four proposed methods described in this paper. Gudulas et al. and Kisi compared the Turc method to other methods, but both papers described that the Turc method is not the most favorable method.

Kisi (2013) compared Turc and multiple other methods with observed evapotranspiration data in a Mediterranean region (Turkey). He concluded that the Turc haves the biggest error compared to the eight other methods. In the four regions used by Kisi, Turc underestimated the evapotranspiration in every single region (ranging from -3.5% to -25%) (Kisi, 2013).

Gudulas et al. (2013) compared the Turc method with the Thornthwaite method. The authors compared these two methods with data from two lakes in Greece. However, Gudulas et al. found that Turc gave higher values than the observed values. To counteract this effect, they propose a correction on the temperature used in the Turc equation. This correction takes into account the annual distribution of precipitation over the course of air temperature. When this correction is used on the annual temperature, more realistic values are estimated (Gudulas et al., 2013).

Taking these two researches into account, a review on whether the Turc method is a representable approach for evapotranspiration is needed (with or without the correction). Since the rescaling of the water balances is of high importance to this research, it is necessary this is done correctly.

SURFACE RUNOFF IN THE CATCHMENTS

As described by Kirchner (2009), surface runoff is not included in the method used in this paper, since it surpasses the storage of the system. Kirchner proposed a new equation to be used for his method. This change in the model was tested for the same catchments, but Kirchner found that precipitation that surpasses the storage was less than 1%, thus resulting in very small changes as compared to the model without the surface runoff. However, this only applies to the catchment used by Kirchner (2009), which was a rather wet catchment. The catchment used by Adamovic et al. is situated within a different climate zone, which is a lot drier that the catchments used by Kircher (2009). In this Mediterranean region, evapotranspiration is higher (especially during summer), resulting in drier soils.

Martínez-Murillo et al. (2006) described the effect of drier soils on surface runoff in a Mediterranean area. After a long dry summer, soils become drier and develop a hydrophobic condition, reducing the infiltration rate. As a result, the overland flow will increase. Burch et al. (1989) described the same effect for soils in Australia, but they discovered that this effect is dependent on the type of soil. Of the two sites studied by Burch et al., only one site developed hydrophobic conditions. At the site without hydrophobic conditions, infiltration was dominated by macropores. The authors compared the site with the hydrophobic conditions to a period where the site did not have hydrophobic conditions, and found that the infiltration volume/precipitation volume ratio was reduced by roughly 50% when the soil developed hydrophobic conditions. This only accounts for the area where the vegetation consists mostly of forest; the grassland showed very little differences.

Furthermore, the amount of surface runoff is also determined by the angle of the slope at which the rainfall occurs. Čustović et al. (2014) studied the effect of different slope angles on the amount of overland flow. Three different slope angles were used. On the bare soils, surface runoff ranged from 8% to 18% of the total precipitation. The slopes with the lowest inclination had the lowest surface runoff, and the slopes with the highest inclination had the highest surface runoff.

Within the Ardeche basin, dry conditions in soils are very likely to occur, due to the higher evapotranspiration rates. Moreover, forest is the most dominant land use type in the Ardeche catchments, which haves the biggest impact on developing hydrophobic conditions.

In section 2.2.1, the four sub catchments are described. All of the sub catchments are characterized by steep slopes (> 15%), so surface runoff is very likely to occur. Just as the effect of dry soils on overland flow, this is not included in the current study. Since the surface runoff could be around 18% of the precipitation (as studied by Čustović et al., 2014), it is important to include in the model.

I would like to see the influences of dry soils and inclination on the surface runoff in the Ardeche catchments. Measuring the surface runoff would be something for different study, but an estimation of the percentage of surface runoff would improve the transparency of this research. .

MINOR COMMENTS

This paper is written very comprehensive, with almost all aspects covered. The text is good to read and contains a lot of information. However, in some sections, I would like to see some more clarification, to reduce any ambiguities. These improvements are described in below.

- There are no clear answers to the research questions as they are proposed in the abstract and the introduction. I would like to see clearly formulated answers in the conclusion.
- To improve the structure of the manuscript, I would like to recommend moving sections 2.2.2 and 2.3 to the methodology section. In these sections, the methods are described to acquire the right data. By moving these sections, the structure and readability of the paper will increase.
- In section 3.1 the method is described. In this section, the whole process towards equation (10)-(12) is described. Lots of references toward Kirchner (2009) are made. However, in the paper of Kirchner (2009) this entire process is described as well, in more detail (in section 3 and 4). Some

formulas are slightly altered (AET instead of E), but this should not be confusing to the reader. A reference towards this paper, stating that the method is already described, will be just as useful. I would recommend leaving equation (10)-(12) in the method section, because these are the most relevant to the reader. When the reader is interested in learning how one came to these formulas, it can be looked up in the paper by Kirchner (2009). I would also recommend leaving equation (14) out of section 3.2, and make a reference towards Kirchner (2009).

- In section 3.4, equation (18) and (19) are not necessary, since both of these equations should be knowledge the reader already haves, or is really easy to find when needed. The range of the values and what values are indicate good performance are useful to present in the paper, since they are more relevant that the equations used to determine these values (standard statistics).
- A clear reason for the four sub catchments is not described (except they had available discharge data). Furthermore, the presented figures mostly present the results for sub catchment #1. In the tables the results from all sub catchments are shown. However, to prevent 'cherry picking,' I would like to see some more graphs from the other sub catchments.
- When reading the paper, I got the feeling the authors underestimated the poor performance of the model during periods with high evapotranspiration. The focus is more on the good performance of the model during wet periods. This study is important, since the catchment is situated in a Mediterranean area (a dry area, especially during summer). So, I would like to see a more detailed description of the poor performance of the model during these dry periods. Additionally, I would like to see a graph that presents the changing NSE through the entire year.
- Through the manuscript, I found that different abbreviations for the Nash-Sutcliffe efficiency are used (NSE and NASH). I recommend using only one abbreviation through the entire manuscript (e.g. NSE).
- In section 5.3.1 and 5.3.2 new results are presented (respectively about 'another storage' and the influence of geology). These results are not described in the results section, and I would like to see some more information about these points in the results.

The figures and tables are well represented, and add a lot to the information described in the text. Just as the text, I would like to see some figures get altered, so the figures become more clear and easier to understand. These alterations are described below.

- In table 6 the errors of the model are presented. I noted big deviations in the PBIAS (not only for the dry year 2005 as described in the text). A PBAIS of ±20% is seen a lot in this table, but little to no attention is paid to these deviations. They all balance each other out, but the model is not able to represent good results for individual years. I would like to see some more clarification about these under- and overestimations, and how these balance each other out.
- In figure 8 and 9, I found that there are too many lines plotted in a single graph. The lines of the parameters shadow the observed hydrograph, making it hard to see which parameter yields the best results. I would recommend plotting only the best value, and the parameter with the lowest and the highest value.

SPECIFIC COMMENTS

In this section, the found typing errors are described.

- p. 10732, line 4; change web-site to website

- p. 10739, line 14-15; defined as a period between sunrise and sunset, should be the other way around
- p. 10748, line 2; change nine-year period to nine year period
- p. 10751, line 23; remove eventually, and place ‘in addition’ at the start of the sentence

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