**Seasonality of hydrological model spin-up time: a case study using the Xinanjiang model (doi:10.5194/hess-2016-316)**

**Author response for reviewer comments #2**

1) What is the authors' working hypothesis for this study? What is the conceptual basis for expecting spinup time to vary based on aridity and season? Why focus on these two factors and not others, such as geophysical or biological conditions?

**Author response:** This study hypothesized that the model spin-up time does vary with timing of the simulation start.

Literature (i.e. Cosgrove et al., 2003; Rahman and Lu, 2015) suggests that soil moisture memory (SMM) time varies spatially and is correlated with precipitation and temperature. SMM affects the spin-up process. A low SMM indicates that the soil moisture anomalies are short-lived and dissipate hurriedly, enabling the model to recover relatively quickly from an undesirable initial state. On the other hand, a high SMM that indicates the slowness of anomaly dissipation and would delay the process of model equilibrium. Koster and Suarez, 2001 and Orth Et al., 2013 explained several controls of SMM (i.e. altitude, slope, soil cover, evapotranspiration, precipitation and runoff) and concluded precipitation and evaporation variability as the prominent control. Since SMM and spin-up time is analogous, this study also assumes that precipitation and evapotranspiration could also play dominant roles for dictating the spin-up time. The non-stationary nature of precipitation and evapotranspiration constructed the conceptual basis for expecting spinup time to vary based on aridity and season?

2) Model spinup can be a significant burden for hydrologic models covering large, distributed regions or highly complex physical process-based models, both of which can be computationally intensive. Lumped and conceptual models are often less expensive to run and therefore spinup time is less of a concern. I recommend that the authors make a better case as to why it is appropriate to use the conceptual Xinanjiang model applied to a small subset of individual basins to answer the broader question of what controls spinup time. If we do not expect spinup times to be similar for conceptual and physical models, would we expect the same climatic or environmental controls?

**Author response:** Considering the differences in model definition and structure, it is quite risky to consider the same control for all the models. Available spin-up literatures are mainly model specific. Lumped and conceptual models are less expensive to run for sure. However, spin-up time could be of a concern for data scarce situation or seasonal simulation (Rahman and Lu, 2015). Common practices consider first 2 or 3 years of simulation as the spin-up period and removes from the rest of the analysis including model calibration. The problem here is to define the spin-up period. This is usually done based on personal feeling, experience, available data records and purpose. In some cases exclusion of one year model outputs could be a very costly task in developing countries where hydro-climatic data is very scarce (say only 2-3 years of available data records). Over-estimating the spin-up period would lead to a loss of important information. Likewise, an underestimation would affect the conclusion by incorporating erroneous initial model outputs.

On the other hand, researchers often consider various spin-up time even for the same model. Lin et al (2016) considered a spin-up period of 19 days for the Xianjiang (XAJ) model during a four-month streamflow simulation for the Shiguanhe River Basin, China. In another study, Lu et al. (2008) considered only 12 h of spin-up time while forecasting floods at the Huaihe River Basin's Wangjiaba sub-basin. Even if the spin-up times are dissimilar for conceptual and physical models, this study serves important information for the XAJ model as well as other modeling communities. Firstly, it provides a basis for estimating the spin-up time for the XAJ model using widely available data sets. Secondly, it establishes a conceptual basis and shows the variations of spin-up time based on the simulation start time that provides new insights even for the physical models (the controls might be different). Thirdly, it ascertains new approaches to explore and define model spin-up time based on broadly acceptable Mahalanobis Distance that over comes the limitations of available spin-up detection techniques.

3) Following from (1) and (2), there are systems that are known to require longer spinup times, such as deep groundwater aquifers, large surface storages, etc. Is the study model configuration capturing any of these slower processes, or purely focused on shallow soil water storage? How appropriate is this model configuration for the study basins' dominant hydrologic regime? How generally applicable are the findings if these processes are not represented? I recommend the authors provide a bit more detail on the model and its appropriateness for the study basins.

**Author response:** The studied model mainly focuses on hydrologically active soil water storage zones. This model is extensively used in humid and semi-arid regions of China and other parts of the world. The runoff formation in the XAJ model is based on the repletion of storage concept, the runoff will start to generate once the soil moisture content of the unsaturated zone reaches its field capacity, and subsequently runoff equals the rainfall excess without further loss (Zhao, 1992). The model accepts areal mean precipitation and pan evapotranspiration as the inputs and produce streamflow from the whole basin. The applicability of the XAJ model of the study basins' have been tested by Kyi, 2014. At this point this study outcome might be true for humid and semi-humid basins of USA. Commenting on the appropriateness of this conclusion at outside USA requires further verification.

4) How does the model calibration affect the results? The calibration procedure is a bit difficult to understand based on the description provided, so I recommend going into a bit more detail on what was done and how it may/may not be impacting spinup times.

**Author response:** The calibration process does not affect the result. The model calibration and exploration of model spin-up time were performed separately (please see page-5, line-11-14). The XAJ model was firstly calibrated with saturated initial condition and thereafter the daily streamflow was validated against those of the observed by taking spin-up time long enough (10 year) to avoid the effects of the initial condition. Thereafter, these calibrated parameter values were exercised for the subsequent simulations that explores the impact of initial condition.

The model calibration section has been updated with more clarification.

5) The authors present some interesting patterns in seasonality of spinup time. However, there is little discussion of the potential physical reasons for the patterns. Are these patterns simply mirroring seasonal patterns of precipitation? Are there other physical or climatic controls that might explain some of the spread, or help us determine how we can apply these results to other models or domains? I recommend expanding the discussion section to address some of these questions, which should give the paper a much broader relevance.

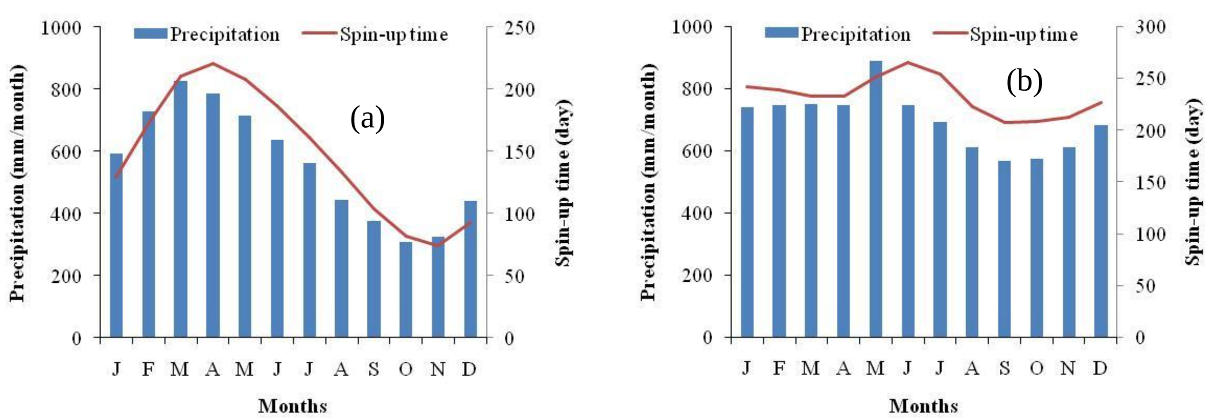


Fig. 1: Seasonality of precipitation and model spin-up time: (a) wet basins and (b) dry basins.

**Author response:** Thank you very much. You are right. Figure 1 suggests that, the seasonality of spin-up time is similar to the seasonality of precipitation. Monthly spin-up time shows strong correlation ($r$=0.87) with monthly total precipitation. The spin-up time was explored by declaring two extreme initial conditions (0% and 100% soil moisture). It is intuitive that the higher the distance between the mean condition and the initial condition, the longer the time requires reaching the equilibrium. The distance between the mean condition and any of the initial condition for the months with higher precipitation (also difference between the maximum and minimum precipitation is high) is wider as compared than that of months with lesser precipitation. Therefore, the higher the monthly precipitation, the larger the spin-up time is.

This discussion will be added into the manuscript.

Specific Comments:
Overall: The paper is nicely organized and figures are clear. However, it would benefit from additional grammar/typo editing throughout the paper.

1. Introduction: Portions of the introduction section (e.g., lines 16-27) read more like methods than introduction. I would recommend moving these specifics on the model strategy to the methods section and dedicate a bit more of the intro to addressing the study hypotheses and rationale.

**Author response:** Thank you very much. Page 3, Line 4-27 have been moved to the section 2.6: definition of model spin-up time.

Introduction section has been improved by incorporating study hypothesis and rationale.

2. Materials and Methods
2.1 Study area: Why choose only snow-free basins? I would guess because the model does not represent snowpack dynamics, but this should be stated. Why these particular 18 basins? The basins are primarily in the South-Central US (with 2 exceptions), not distributed across the US. I recommend describing the hydrologic regime in this region so we understand some of the seasonal patterns better – what is the seasonality of precipitation? Is there deep groundwater storage? What controls the runoff response?

**Author response:** The basin characteristics (including hydro-meteorological) are given in Table 1. The studied river basins were selected intentionally mainly for two reasons.

Firstly, to maintain consistency with Rahman and Lu (2015) and Rahman et al. (2015) as they discussed model-spin-up time and soil moisture memory for the same river basins. Rahman and Lu (2015) discussed model spin-up time based on a different methodologies (single year recursive simulation) that of ours (multiyear climatologies). We prefer to analyze same river basins for comparing model spin-up outcomes derived from two different methodologies. On the other hand, Rahman et al. (2015) analyzed soil moisture memory (SMM) for the same river basins. Therefore, it enables to relate the model spin-up time and soil moisture memory. Since, soil moisture autocorrelation equation (based on what SMM was estimated) does not consider snow, Rahman et al. (2015) choose snow free MOPEX basins for analysis and ultimately led the selection of studied basin for the present study.

Secondly, experience suggests that the XAJ model does not produce better results for all the MOPEX basins, particularly for the drier basins (Kyi, 2014).

2.2 Xinanjian Model: The model assumptions and configuration are fairly important for this study, so I think more detail on the model description is warranted. For example, it is not clear whether this is a lumped or distributed model when applied to the individual basins. What types of hydrologic systems does the model perform well in, and what types do poorly with this conceptual representation?

**Author response:** The XAJ model description section has been improved.

The XAJ works as a lumped model while applied to the individual basins. It works better under humid condition and poor under arid condition.

2.4 Parameters, Calibration, Validation: The calibration/validation procedure is not really described here, and is only vaguely described in the next section. I recommend expanding this section to detail the calibration/validation procedure so the reader can understand the potential sensitivity of the spinup results to the calibration.

**Author response:** Calibration and validation procedure has been clarified.

2.5 Simulation Design: Per the previous comment, it is difficult to disentangle the calibration procedure and the spinup procedure based on the description provided. I recommend separating the descriptions and clarifying the procedures.

**Author response:** Descriptions has been separated and clarified accordingly.

3. Results and Discussion: Per the general comment, I recommend adding discussion on the possible physical reasons for some of the observed patterns. As written, this section is really just results. SMM and its calculation should be better defined. The results really need to be related back to the model assumptions, climate regime, or physical basin characteristics to be relevant to other studies.

**Author response:** Result and discussion section has been improved.

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