Comments from Referee #2

We thank the reviewer #2 for his/her thorough comments on our manuscript. Please find below a detailed feedback to individual comments and questions

1. The introduction presents a cursory outline of existing work on hydrological regime shifts. Importantly, a shift in runoff per unit rainfall (e.g. Saft et al., 2015) is not evidence a regime shift to an alternate attractor. Doing this requires cessation of the disturbance and evidence of non-recovery, as demonstrated by Peterson et al. (2021).

Yes we completely agree that a shift in runoff per unit rainfall is not evidence of a regime shift to an alternate attractor. In the introduction of this MS, we define a regime shift as the transition from an attraction basin to an alternative attraction basin.

During the outline of existing work on hydrological regime shifts, we cite Saft et al. (2015) and Peterson et al. (2021) as hydrological studies that “rely solely on data to emphasize multiple lines of evidence that could be related to a regime shift”. In the revised MS, we will remove the erroneous statement “rely solely on data”. Indeed studies such as Peterson et al. (2021), do not rely on attractors and a dynamical model, but on statistical models (HMMs) and cannot be categorized as “relying solely on data”.

As reminded by the reviewer, multiple lines of evidence can suggest the existence of a regime shift, i.e. the shift to an alternate attractor. These evidence include i) a shift in runoff per unit rainfall, ii) a cessation of the disturbance, and iii) an evidence of non-recovery. In the introduction of the revised MS, we will detail these multiple lines of evidence for Sahelian watershed: the cessation of the disturbance (the drought) was analyzed in Panthou et al. (2018), while the two other lines of evidence are studied in Descroix et al (2009 and 2018). These lines of evidence are analysed in a companion paper which is under review. In the revised MS, we will cite this companion paper if it gets published in the meantime. In this MS, we focus on a complementary line of evidence based on a dynamical model.

Additionally, the mechanisms for regime shifts is vague (Peterson et al., 2012), the role of forcing on regimes is not examined (Peterson et al., 2014; Peterson and Western, 2014) and informative case studies using numerical hydrological models are overlooked (Anderies, 2005; Anderies et al., 2006).

Yes, we missed these additional references from Australian hydrology. In the revised MS, we will include references to these articles in the introduction.

- (Anderies, 2005, Anderies et al., 2006) Both articles extract unstable and stable equilibrium from a minimal dynamical model applied to a catchment.
- (Peterson et al., 2012) extract attractors, repellors and attraction basin for a simple lumped groundwater model.
- (Peterson and Western, 2014, Peterson et al., 2014) This two-part article relies on an ecohydrological model to investigate the existence of multiple stable-states.

Additionally, the many efforts by others to identify the timing of shifts (Peterson et al., 2021) [...] is overlooked [...].

In the article (Peterson et al., 2021), that we read and cited in the preprint, we initially did not notice that they were also identifying the timing of shifts. But indeed, in Figure S19 and S20 in the Supplementary Material, there are two graphs where we can see the conditional state probabilities over time, and the year when the probability crosses 50%. In S19 there is a switch from normal to low state, while in S20 there is a switch from high to normal state.

In the revised MS, we will mention in the introduction that other works have previously identified the timing of shifts and cite (Peterson et al., 2021). This finding does not make our contribution outdated, since in (Peterson et al., 2021) regimes are identified statistically, while in our MS the regimes are identified with the bifurcation diagram.

Additionally, the many efforts by others to identify [...] the hydrological mechanisms is overlooked (Fowler et al., 2016, 2020; Saft et al., 2016).

Thank you for all these additional references, from Australian hydrology, that we missed.

- (Fowler et al 2016) analyzes from the literature that conceptual rainfall-runoff models are leading to poor performance when evaluated over multiyear droughts.
- (Saft et al 2016) asks whether model performance degradation is due to climate shift only or to shifts in internal catchment functioning
- (Fowler et al 2020) points out that conceptual rainfall-runoff models should be improved to account for long and slow dynamics, i.e. storage/memory effects.

In the introduction of the revised MS, we will include references to these articles.

2. The MS asks did these regime shifts occur? Given well established statistical models and code exist for this, e.g. Hidden Markov Models (Peterson et al., 2021), it is very unclear why the proposed approach is appropriate.

We agree that Hidden Markov Models (Peterson et al. 2021) could be adapted to study the timing of regime shift. However, studying regime shifts and their timing requires multiple lines of evidence. Here, we propose an original and complementary line of evidence, based on a dynamical model, which seems relevant and appropriate. Indeed, in our MS, regimes are identified from a bifurcation diagram (and therefore correspond to the classical definition of regime shift in system dynamics), while in (Peterson et al., 2021) the timing of regime shift is also investigated but regimes are identified statistically from data.

The MS may be misleading, since we do not ask whether these regime shifts occured. The occurrence of a regime shift is a starting assumption, based on early works such as Panthou et al. (2018), Gal et al. (2017) and Descroix et al. (2009, 2018). This MS rather asks: when did these regime shifts occur ? As detailed in the introduction, “for every watershed, we assume that this Sahelian hydrological paradox corresponds to a hydrological regime shift from a low to a high runoff coefficient regime, and ask: when did the shift between these two regimes occur ?”.

3. The use of an ODE to identify attractors etc is interesting. The ODE developed, however, lacks a clear hydrological basis and does not draw on well established hydrological processes. Overall, it appears to be drawn from the school of ecosystem resilience that has for too long relied on toy models that are incapable of explaining observations or offering practical insights (Newton, 2016). I urge the authors to develop a model that is based on hydrological mechanisms.

We recognize that it could appear at first as developed from the school of ecosystem resilience, however as stated in the MS, the ODE is drawn from the school of ecological modeling, which can sometimes involve studies on resilience but not necessarily. This MS does not focus on resilience at all.

The co-authors of this MS have decades-long experience in tropical eco-hydrology, gained in the field and with process studies (e.g review in Lebel et al, 2009, Galle et al, 2018). Many of them are also very familiar with physically based models, as a developer or an advanced user (e.g. Casse et al, 2016, Gal et al, 2017; Getriana et al, 2017 ; Hector et al, 2018). We deliberately chose to develop a minimal model capable of reproducing the first-order processes (Scheffer and Carpenter, 2003). In this data-scarce region, the comprehensive dataset required to inform a more complex, physically-based model throughout the investigation period is not available, especially before the satellite era. Therefore our model is consistent with both the available datasets and the basic assumption of the work that a regime shift occurred and that drought was a sufficient trigger. To the best of our knowledge, no formalism capable of representing the feedbacks we introduce have been published. Following the parsimony principle of model development, we started with a simple model, aiming at improving our understanding of these overlooked interactions which drive such complex eco-hydrological systems (Sivapalan 2018).

The model has been built from the most recent state of knowledge about eco-hydrological processes in Subsaharan Africa. Although the model equations are not properly based on physics, they describe real hydrological mechanisms using response functions (e.g. Eq. 1, K vs P relationship, see also answer to Rev #1 about this equation). As described in the methodology section, most variables and parameters of the model can be assigned an hydrological meaning. Therefore, we do not agree that it is a “toy-model”. Compared to theoretical studies with toy-models, where models are forced with long simulated time series of constant external forcing, and where the parameters are not really constrained, our approach goes one step beyond by confronting the model with observations. Any model with stationary internal basin properties - e.g. by neutralizing the feedback loop in our model- fail to reproduce the observations (see new data in answer to Rev #1). Our model reproduces the trends (or first-order dynamics) in the observations, based on the assumptions upon which it was built. In this sense we consider it “explains” the observations. However, like any other model, it is considered relevant until its skills degrade or one assumption is disproved. Hence we claim that our modelling framework is an adequate compromise between complexity and performance, for the emerging issue of regime shift investigations.

4. The approach for identifying the steady state regimes (i.e. attractors) and the fold points is very problematic. The MS presents an ODE but then uses time-solutions rather than bifurcation (Eq 4). Very well established analytical and numerical methods exist that estimate stable and unstable states with a forcing variable and then also the fold points, i.e. the thresholds between states. I urge the authors to look at these hydrological studies (Peterson, 2009; Peterson et al., 2012; D'Odorico and Porporato, 2004; D'Odorico et al., 2005), worked examples (Ludwig et al., 1997) and mathematical references (Kuznetsov , 2004; Dhooge et al., 2003). Using such methods, it should be possible to probabilistically quantify the regimes and thresholds with significantly more confidence.

In the MS we rely on time-solutions with two different initialisations, following the methodology used in a previous article from our group (Wendling et al. 2019). Thank you for the reference of Dhooge et al (2003), which refers to a Matlab package for numerical bifurcation analysis called MATCON. Indeed, such a package for bifurcation analysis (based on continuation method) seems more adapted to estimate stable and unstable states.

In the revised MS, we re-compute stable and unstable states using a Python package for numerical bifurcation analysis called "pycont-lint", which is also based on a continuation method. We consider this is a considerable improvement in our methodology. Such a continuation method requires starting from a known solution. In practice, we initialize it with a state value near 0 (S=0.01) for a forcing precipitation close to 0 (P=0.1). Then, one iteration later the algorithm starts following the isoline such that dS/dt = 0. We stop the continuation method when the trajectory goes beyond a precipitation level equal to 4000 mm. Finally, as a postprocessing step, we linearly interpolate the trajectory to obtain the value of stable and unstable states only for integer values of precipitation (1, …, 4000).

In the Figure below (Fig. 1), we show an updated version of the bifurcation diagram shown in the MS. We observe that the attractors are similar. Thanks to the continuation method we also have access to the unstable state, i.e. repulsor, displayed with a dotted line here.

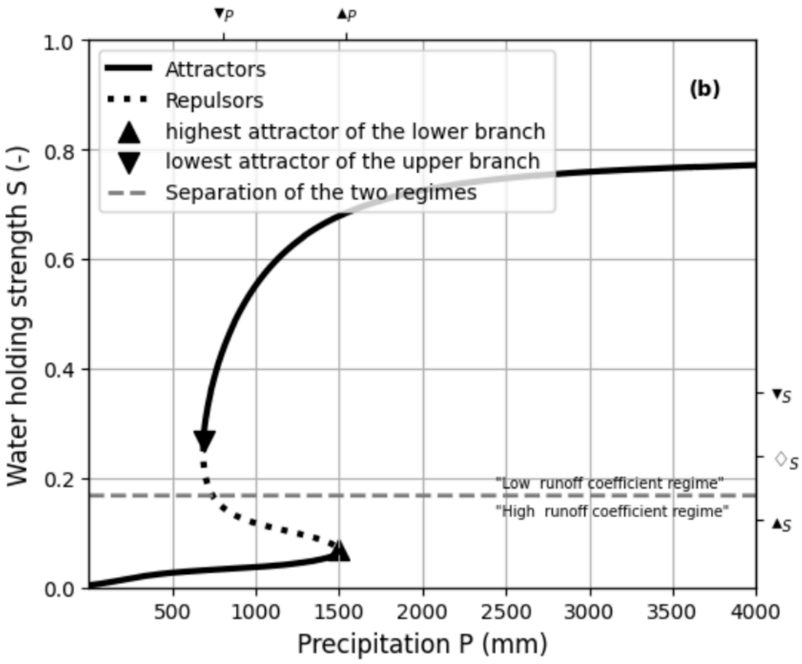

Fig 1: Updated version of the bistable bifurcation diagram from the manuscript (Fig 4 a) with the novel methodology to compute attractors.

5. Section 3.2 states that the ODE is calibrated. This is not correct. The MS samples the parameter space but does not use any objective function to either reject implausible parameters (e.g. GLUE), or estimate formal likelihoods (e.g. Vrugt, 2016). Similarly, the approach cannot also be called sensitivity analysis, given the lack of quantitative evaluation against observed flow.

In the MS we do not state that "the ODE is calibrated", but that we calibrate an ensemble of parameterization. We agree with the reviewer that the word "calibration" may not be adapted to our approach. Indeed, a calibrated ensemble would ideally be selected by minimizing an objective function that evaluates the quality of an entire ensemble, so that selected parameterizations are complementary/well-combined. In our approach, this is not the case, as parameterizations are selected independently/separately.

In the revised MS, we will remove the term "calibrated ensemble", and we will rename "calibration of the model" by "selection of the best parameterizations of the model".

To the best of our knowledge, in the context of non-autonomous dynamical models, such calibration of an ensemble, e.g. based on a formal Bayesian approach, would likely be extremely difficult. Let $\mathbf{K} = [K_{t0},\ldots,K_t]$ be a time series of observed runoff coefficients, and $\boldsymbol{\theta}$ a parameterization. To compute/estimate the posterior $p(\boldsymbol{\theta}|\mathbf{K})$ we would need to define the prior $p(\boldsymbol{\theta})$ and the likelihood $p(\mathbf{K}|\boldsymbol{\theta})$. Defining the joint prior distribution $p(\boldsymbol{\theta})$ would involve choosing marginal distributions for each parameter and a dependence structure between them. For most of them this dependence is unknown. Decomposing the likelihood $p(\mathbf{K}|\boldsymbol{\theta})$ would need some assumptions. Generally, conditional independence between successive time steps is assumed, such that $p(\mathbf{K}|\boldsymbol{\theta})$ can be decomposed as the product of $p(K_t|\boldsymbol{\theta})$. However, in our context this assumption would likely be erroneous because there is a strong temporal dependency. One solution could be to rely on a hidden Markov model, like Peterson et al. (2021), but this would likely require many additional assumptions. Although intellectually more satisfying, a formal Bayesian approach would need a series of assumptions whose justification is all but obvious. For all these reasons, we retained a simpler approach.

The claim that our approach does not use "any objective function to reject implausible parameters" suggests we have to clarify our method. In our methodology, the selection of the best parametrizations, which amounts to rejecting the implausible ones, relies on the root mean squared error between observed and simulated runoff coefficients. The mean squared error (MSE) is a standard objective function in statistics that can be interpreted as maximum likelihood estimation, when one assumes that the target distribution is Gaussian (Sect 5.5.1 of Goodfellow et al. 2015).

Additionally, the predictor (rainfall) is not independent of the predicted variable (i.e. runoff ratio) because it is used in the denominator of the runoff ratio. I urge the authors to develop a formal likelihood function for flow (not runoff ratio) and then do MCMC estimation of the parameter uncertainty.

The term “predictor” and “predicted variables” are not used in the MS, and are misleading in this context, as they refer to a statistical learning framework. Here rainfall is an external forcing for a dynamical model that simulates the runoff coefficient. For such an non-autonomous dynamical model, there is no issue that the simulated variable is dependent on the external forcing variable, with the exception that this dependence might make calibration/fitting more difficult. Here the model does not predict runoff coefficient for a given year using only rainfall of the same year. As shown in Eq. 1, the runoff coefficient for a given year depends both on rainfall P of the same year, and on the state variable S (through the variable C). The value of the state variable S does not depend only on the rainfall of the current year, but also on the previous values of S, hence on the whole rainfall history (through Eq. 3).

In many articles on hydrological regime shift, the watershed outflow is modelled because it is the key physical variable enduring a regime shift (usually from flow to low flow). However, for the considered Sahelian watersheds, we observe a hydrological paradox (assumed to be a regime shift in this MS) with respect to the runoff coefficient. Thus, this is the reason why our dynamical model focuses on modelling the dynamics of the runoff coefficient.

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