We thank the reviewer #1 for his very thorough comments on our manuscript. Please find below a detailed feedback to individual comments and questions.

Lines 45–50: You introduce a model with precipitation as the *sole* external forcing. While conceptual simplicity is appreciated, the omission of known key drivers like land use changes (e.g., deforestation, cropland expansion, crust formation) and rainfall intensity is somehow concerning. Given existing knowledge on Sahelian hydrology, this simplification may lead to misleading causal inferences. The rationale for excluding these variables needs to be better justified, and at minimum, the implications of this omission must be critically discussed earlier.

The main assumption of this work is that a regime shift occurred in the Sahelian watersheds. Following Wendling et al (2019), we also assume (implicitly in our methodology) that the prolonged rainfall deficit was a sufficient driver to trigger this shift. Indeed, we introduce a model solely forced by rainfall, first to check that regime shifts can be reproduced using bifurcation diagrams, and to assess the timing of the shift.The main rationale for excluding key processes of Sahelian hydrology (land cover changes, rainfall intensity) is to test our unusual hydrological modelling approach in successive phases of increasing complexity. We start first with rainfall, which is the dominant driver of hydrological behaviour at the watershed scale, as a proof-of-concept of the approach. In the model, we note that soil crusting is implicitly accounted for with terms 1 and 2 in Eq. 3, which mimic the increase in runoff-prone areas in conditions detrimental to vegetation development, and the expansion of these areas when they are already present.

Therefore, yes, omitting land cover changes and rainfall intensity restricts the scope of our results, and any result should be understood within the framework of the underlying hypotheses and design choices. In the conclusion of the manuscript, we reiterate that "These results depend on several design choices of our two methodological contributions". In the revised manuscript, we will emphasize our main hypotheses earlier in the abstract and recall it later in the conclusion. Specifically, when we state that "the year of the regime shift was 1968, 1976, 1977, 1987 for the Gorouol, Dargol, Nakanbé and Sirba watershed, respectively." We will add afterwards that "these results should be taken with caution because they were obtained with a parsimonious model which neglect other important processes of Sahelian hydrology. It would therefore be wise to supplement this analysis with other models — with varying levels of complexity — that allow regime shifting. "

Causal inferences will be considered once our model includes all the key processes of Sahelian hydrology. Some work is underway to include more processes in the model:

1. Following an internship, there is on-going work to modify the model and to rely on daily rainfall to design annual indicators to substitute annual rainfall as forcing.

2. In early trials, we integrated vegetation as an additional constraint to calibrate an almost similar dynamical model. However, satellite observations were limited in time and did not show much temporal changes, which made calibration difficult.

3. Including land cover changes is challenging as in this region, such changes are related both to endogeneous dynamics, as observed in the northern areas, (represented in our model with feed-back mechanisms) and to anthropogenic land clearing mostly for expanding rain-fed crop surfaces. The methodological issue is to cope with two external forcings (rainfall and land cover) simultaneously. Techniques such as nudging (to relax the model to an observed trajectory without totally constraining its dynamics) will be considered.

Overall, we are convinced that multiple lines of evidence are required to study hydrological regime shifts of Sahelian watersheds. This manuscript provides one line of evidence (with indeed some conceptual simplicity) based on a dynamical model and its bifurcation diagram. Complementary, in a companion paper under review, we also study the statistical links (without a dynamical model) between rainfall, vegetation and runoff coefficient. In the revised manuscript, we will cite this companion paper (if it is published in the meantime) to explain how the current manuscript supplements it.

Also, not all readers are familiar with the term "attraction basin", which needs explaination beforehand.

To clarify the term "attraction basin", we have rewritten the first paragraph of the introduction in the revised manuscript as follows: "Complex dynamical systems (ecosystems, climate system) can have, for certain external conditions, several attractors (stable states) towards which the system state converges depending on its initial value. The set of initial values converging to the same attractor defines an attraction basin. Classically, one regime is associated with each attraction basin (Mathias et al., 2024). A regime shift, i.e. the transition from one regime to the alternative regime, corresponds to the crossing of a tipping point: a critical value beyond which a system switches to the alternative regime, often abruptly and/or irreversibly (IPCC, Annex VII: Glossary, 2023). "

Lines 100–110 (Eq. 1 and surrounding text): The functional form used to relate S, P, and K is not adequately justified from a physical or empirical standpoint. For instance, the role of the parameters aa and bb in shaping the runoff response curve requires clearer explanation.

The relationship between K, S and P is a fully derivable variant of the popular SCS model (Mockus, 1972), and of the equation used by Massuel et al. (2011) to simulate runoff in the Sahel. It is also very similar to the formalism used by Anderies et al (2005, Eq. 20) to represent the dependence of runoff to rainfall and watershed-scale water retention capacity, analogous to our variable "C". This S-shape function models the main features of the rainfall-runoff relationship: no runoff is produced below a certain rainfall amount, the runoff ratio varies only little for heavy rainfall, and it increases roughly linearly for intermediate rainfall. The parameters a and b are shape factors allowing the model to adapt to a wide range of watersheds. The mathematical form of the equation is well adapted to ODE solving. Although it is not properly a physically based model, it provides a sound representation (i.e. consistent with known hydrological processes) of the dependence of the watershed runoff ratio K to the rainfall P, for a given – time-varying – infiltrability state of the watershed (C). We acknowledge that these justifications are lacking. A more detailed description of the model principles will be included in the revised manuscript.

Why not explore more physically based alternatives or benchmark this against empirical runoff-precipitation relationships?

We agree that the same kind of analyses have to be done with other types of models to support multiple lines of evidence about hydrological regime shifts. Currently available hydrological models poorly represent regime shifts (Avanzi et al, 2020, Fowler et al, 2022), mainly because they do not include the key feedback processes.

Implementing such feed-backs in existing models is a great challenge as it potentially requires revising the whole model structure, all the more so for complex physically-based models. This is why we chose a simpler, semi-empirical approach. Another important reason is that the datasets required to inform more complex, physically-based models, are not readily available and/or non-existent at the appropriate scale for the 1950s-1980s decades.

In the Appendix of the revised manuscript, we will include a benchmarking against the model GR1A Mouelhi et al. (2006), adapted to the annual time step that does not have a feed-back loop. In the Figures below, we illustrate the tuning of this model on the four watersheds. For each watershed, we tune its sole parameter X (constant over time) based on the runoff coefficient observations (Obs) displayed as white dots on the Figures. Simulated runoff coefficients are displayed as red dots. Based on these 4 graphs, we observe that this model fails to model the trend of runoff coefficients.
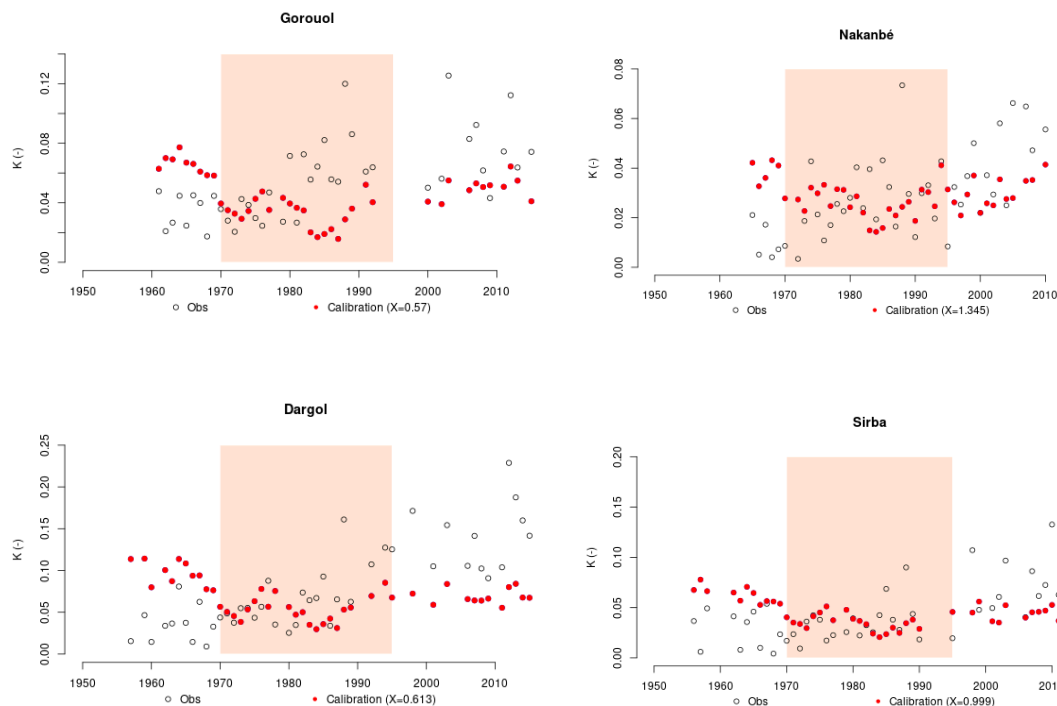
Previous modelling studies on Sahelian watersheds successfully reproduced observed runoff or runoff ratio only when land cover changes (which drive the hydrological response to rainfall) were prescribed (Gal et al, 2017, Casse et al, 2014, Yonaba et al, 2021). This is why we tentatively introduced a dynamical watershed state parameter with a simple model structure.

We acknowledge that the current equations may be misleading. Due to their endorheic nature, most Sahelian watersheds have sub-domains that do not contribute to runoff at the outlet. In order to account for this partial contribution, we introduced f, defined as the fraction of the watershed which actually contributes to watershed outlet. The model only focuses on the contributing areas, which are also the areas where a regime shift can occur. In the current form of Eq 1, K is the runoff ratio of the whole watershed, and the term multiplied by f represents the runoff ratio of the contributing areas. Eq. 2 computes the wetness index from the runoff ratio of the contributing areas, which explains the term K/f.

In the revised manuscript, we will rewrite Eq. 1 as follows, where K* will be defined as the runoff ratio of the contributing areas only. Eq. 2 will be rewritten accordingly, Eq. 3 will be kept unchanged, and we will introduce Eq. 4, where K is the whole watershed runoff ratio. Eq. 4 is needed to scale the runoff ratio between the contributing areas and the whole watershed, and compare with the observations. These equations are strictly equivalent to the current ones, but more easily readable.

$$K^* = k_{max} \cdot \left(\frac{P^a}{P^a + C^a} \right)^b \text{ with } C = c_{min} + S \times (c_{max} - c_{min}) \tag{1}$$

$$I = P \cdot (1 - K^*) \tag{2}$$

$$\dot{S} = \overbrace{r_g \cdot \frac{I}{I + i_g} \cdot S \cdot (1 - S)}^{(1)} - \overbrace{r_d \cdot \frac{i_d}{I + i_d} \cdot S}^{(2)} + \overbrace{\mu \cdot (1 - S)}^{(3)} \tag{3}$$

$$K = f \times K^* \tag{4}$$

The range of values of μ (2e-3 ; 5e-3, Table 1) are 2 to 3 orders of magnitude lower that the growth and decay rates r_g and r_g, and the term $\mu(1-S)$ remains negligible as compared to the other terms of Eq. 3, except when S becomes close to 0 (in this case terms 1 and 2 in Eq. 3 are also close to 0). Thus, this term cannot affect long-term trajectories, and is designed to play a role only when S is close to 0 (this term avoids the trajectory to remain stuck in 0).

Lines 115–125 (Model calibration): While you cite equifinality to justify using an ensemble, you could improve the reproducibility of your methodology by clarifying the basis for choosing the "top 1,000" parameter sets. Why not explore a weighted ensemble or Bayesian approach to deal with parameter uncertainty more formally?

This methodology is reproduced from a previous publication (Wendling et al. 2019) where they sample 1 000 000 parametrizations and select the best 1000 (to be transparent, this publication has many co-authors in common with this article). Computational cost is the main reason for choosing the best 1000 parametrizations, as for each selected parameterization we need to compute 2000 attractors (between 1 mm and 2000 mm). However, we agree that there is no formal justification/basis for choosing the best 1000.

We discuss below the two proposed methodologies to explore uncertainty more formally.

- A weighted ensemble could be a possible extension. However, this would require an additional assumption to compute the weights for each selected parameterization. For instance, we could choose weights as being inversely proportional to the RMSE. Such an assumption would put more weights on the parameterization with small RMSE. This is not necessarily desirable. In our current approach, the choice of an ensemble without weights, i.e. the most parsimonious choice, implies the assumption that every selected parameterization is considered as equally plausible. This large variety of parameterization, which brings confidence in our results, is also enforced by the fact that the parameterizations are sampled using a Latin hypercube sampling, that ensures a relevant exploration of the parameter space.

- A formal Bayesian approach would correspond to a weighted ensemble, where the weight of each parameterization θ is the posterior $p(\theta|K)$, where $K = [K_{t0},\ldots,K_t]$ is the time series of observed runoff coefficient. However, to the best of our knowledge, in the context of non-autonomous dynamical models, such a formal Bayesian approach would likely be extremely difficult. Indeed, to compute/estimate the posterior $p(\theta|K)$ we would need to define the priori $p(\theta)$ and the likelihood $p(K|\theta)$. Defining the joint prior distribution $p(\theta)$ would involve choosing marginal distributions for each parameter and a dependence structure between them. Decomposing the likelihood $p(K|\theta)$ would need some assumptions. Generally, conditional independence between successive time steps is assumed, such that $p(K|\theta)$ can decompose as the product of $p(K_t|\theta)$. However, in our context this assumption would likely be erroneous. Anyway, designing $p(K_t|\theta)$ and choosing hyperparameters would have been difficult. For all these reasons, we did not choose to explore a formal Bayesian approach.

Following a suggestion from the reviewer #2, in the revised manuscript we compute attractors with a more advanced methodology, a method based on numerical continuation, which removes the arbitrary choice of 10,000-year simulations. Specifically, we rely on a Python package for numerical bifurcation analysis called "pycont-lint" (a sub-package of PyDSTool). This 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. Note that we have tested the sensitivity of the algorithm to the choice of initialization. It appears that the initial value of S has no effect on the first iteration as long as S remains below 0.1.. The iterations of the continuation method are stopped 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 stable and unstable states for integer values of external forcings, i.e. precipitation equal to 1 mm, …, 4000 mm.

In the Figure below (Fig. 1), we show an updated version of the bistable bifurcation diagram from the manuscript. We observe that attractors are similar. However, thanks to the continuation method we also have access to the unstable states (repulsors) displayed with a dotted line. In the revised manuscript, for clarity, we will only show the bistable bifurcation diagram between 1 mm and 2000 mm (even though the continuation is run until 4000 mm).
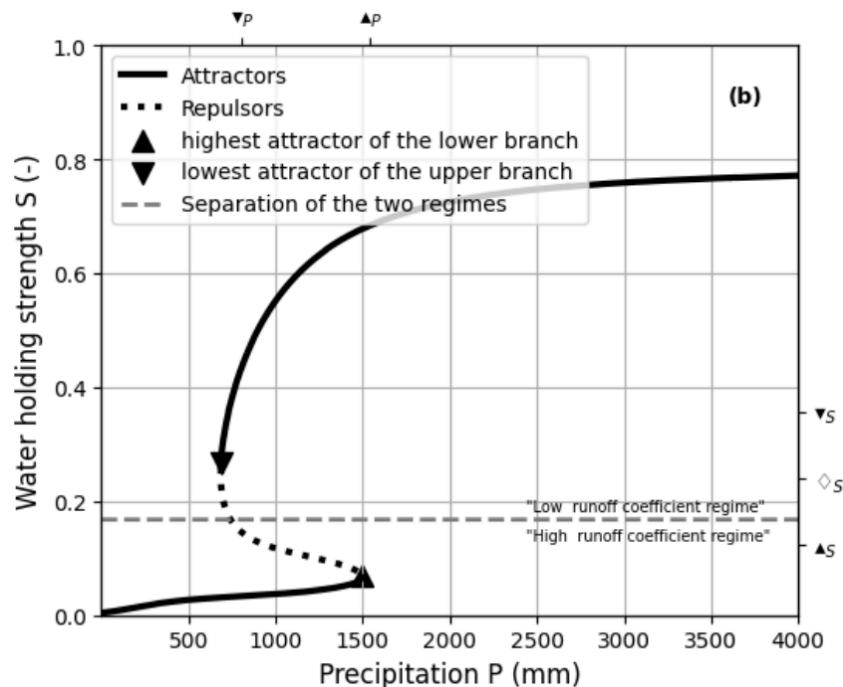

Fig 1: Updated version of the bistable bifurcation diagram from the manuscript (Fig 4 a) with the novel methodology to compute attractors.

Next, we assess the sensitivity of bistability to the maximum threshold to compute attractors (2000 mm in the submitted manuscript, 4000 mm in the revised manuscript). Specifically, we check the percentage of bistability for other thresholds taken every 10 mm.

In the revised manuscript, we will define the attractors with the most advanced methodology (as explained above). In this case, in the Figure below (Fig. 2), we observe that between 1500 mm and 4000 mm the percentage of bistable ensemble members is almost constant for three watersheds (Gorouol, Dargol, Nakanbé). Thus, the number of bistable ensemble members is insensitive to the maximum threshold, as long as it is above 1500 mm. For the Nakanbé watershed, the percentage of bistable ensemble members is sensitive to the threshold (the percentage of bistability is increasing with the threshold). However, thankfully, our new definition of "regime shift year" (see below our answer to **Lines 180–190 and Figure 7**), which does not depend anymore on a percentage of bistable ensemble members that shift, implies that the sensitivity of the Nakanbé watershed will have little impact impact of the main conclusion of this work (dates of regime shifts).
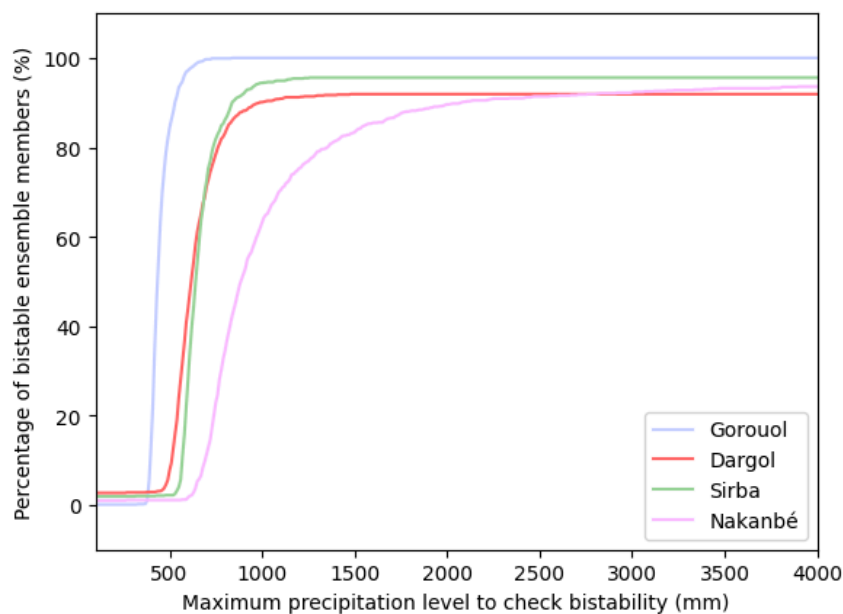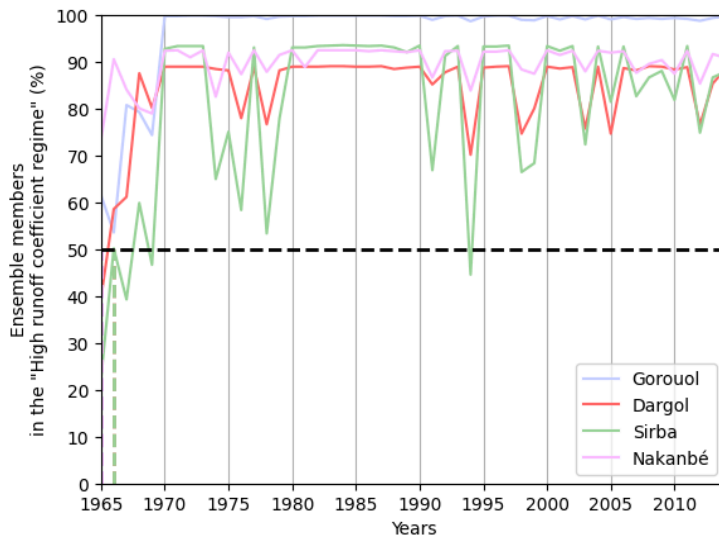

Fig 2: Updated version of Figure 7 from the manuscript with the novel methodology to compute attractors.

Lines 145–155 (Eq. 4 and definition of regime): The operationalization of regime shifts via S=(↑S+↓S)/2 is a critical assumption. This midpoint criterion might not capture the actual dynamics of transition in transient regimes. Alternative definitions (e.g. basins of attraction) or at least a justification for this heuristic are needed.

Yes, alternative definitions of regimes can be imagined. Following the vocabulary from Mathias et al. (2024), our definition based on a threshold corresponds to the "definition of regime based on a norm", where regimes are separated by an horizontal threshold. Another possibility is to rely on a "definition of regimes based on attraction basin". The reason why we choose the first definition is that it is more intuitive: any state value S can be sorted directly in the low or high regime by comparing it with some threshold. On the other hand, with a definition of regimes based on attraction basins, a state S cannot be directly sorted in one regime, as the definition of regimes depends also on the precipitation P. Thus, the same state S can switch regimes just because the external forcing precipitation has changed. Therefore, with the definition based on attraction basins, we would expect that the trajectory switches more between low and high regimes.

Thanks to the more advanced methodology to compute attractors, we have access to repulsors (see previous answer) and therefore we can define regimes based on basins of attraction. In the Figure below (Fig. 3), we show the evolution of the percentage of ensemble members in the high regime. In this case, we find that the year of the regime shift (first year where more than 50% of ensemble members are in the high regimes) would be before the drought (and often before 1965). This is due to the fact that for every watershed many ensemble members are in the high regime in 1965.

Fig 3: Updated version of Figure 7 from the manuscript with the novel methodology to compute attractors and using a definition of regimes based on attraction basin.

In the revised manuscript, we will keep the initial definition of regimes for two reasons: it leads to less "switching between low and high regimes" (which seems more plausible) and it is more interpretable as any state value has a single regime (whereas, with regimes defined by attraction basin, some state value can sometimes have two possible regimes depending on the associated value of precipitation P). We will add an Appendix showing the results for the definition of regimes based on attraction basins.

Figure 5 / Lines 155–170: The mismatch between observed runoff coefficients and the simulated ensemble spread—especially for the Sirba and Nakanbé basins—is troubling. It casts doubt on whether the model can adequately capture year-to-year variability or nonlinear transitions. Please provide a quantitative assessment of performance beyond RMSE (e.g., Nash-Sutcliffe efficiency, bias).

We acknowledge that the model cannot simulate year-to-year variability. It has not been designed for that purpose, but rather to restitute decadal dynamics as observations show that the non-linear transitions responsible from the regime shifts operate at such time scales.

In the revised manuscript, we will include a Table in the Supplementary with the quantitative assessment as advised by the reviewer. For each watershed, we computed the RMSE, bias, and Nash-Sutcliffe efficiency (NSE) for the top 1000 parameterizations. The fit is deemed as "good" for Dargol Kakassi (all NSE are larger than 0.7), "satisfactory" for Sirba (all NSE larger than 0.54), and "unsatisfactory" for Gorouol (NSE are between 0.43 and 0.53) and Nakanbe (NSE are between 0.45 and 0.5). For these two latter cases, the absolute bias remains below 0.01. These measures are consistent since the variance of the simulated values is lower than that of the observations, hence leading in some cases to poor model performance regarding NSE, while the simulated trajectory does minimise the bias. In the manuscript, we acknowledge that the model cannot simulate year-to-year variability, mainly because forcing the model with annual rainfall does not account for the non linear dependency of runoff to rainfall at shorter time scales, and because other factors such as anthropogenic land cover changes have been omitted. This was well known from the beginning, but it did not affect the main objective of reproducing the first-order (i.e. on decadal time scales) hydrological dynamics..

Lines 180–190 and Figure 7: The definition of the "regime shift year" as the first time when more than 50% of ensemble members enter the high regime seems arbitrary. Why not use a probabilistic or statistical breakpoint analysis? The current criterion could lead to inconsistencies in estimating regime shift timing, as seen in the Gorouol case.

Thanks to the reviewer #2, we realized that our definition of "regime shift year" has already been proposed in the literature, see. Figure S19 and S20 in the Supplementary Material of Peterson et al. 2021. However, yes in the Gorouol case the current criterion/definition of the "regime shift year" can lead to inconsistencies.

Probabilistic or statistical breakpoint analysis does not seem to be adapted to our context. To apply such analysis, we could replace every ensemble member by 0 if the shift did not occur and by 1 if the shift occurred, and then apply some breakpoint analysis test. However, this definition and the results would not be really interpretable.

Following the advice of the reviewer (to avoid inconsistencies as seen in the Gorouol case) in the revised manuscript we propose a simpler definition of the "regime shift year": the year when the number of regime shifts is maximized. Visually, it corresponds to the year where the slope of the percentage of ensemble members in the "High regime" is maximized. In practice, we find the year where "percentage(year+1)-percentage(year-1)" is maximized.

In the Figures below (Fig. 4, Fig. 5), we compare the old definition of "regime shift year" with the new definition in two cases: with the attractors of the submitted manuscript, with the attractors of the revised manuscript.

- Attractors of the submitted manuscript. With the new definition of "regime shift year", we find that the shift occurred in 1972, 1973, 1984 for Nakanbe, Dargol, Sirba, respectively. For Gorouol the shift was found in 1971
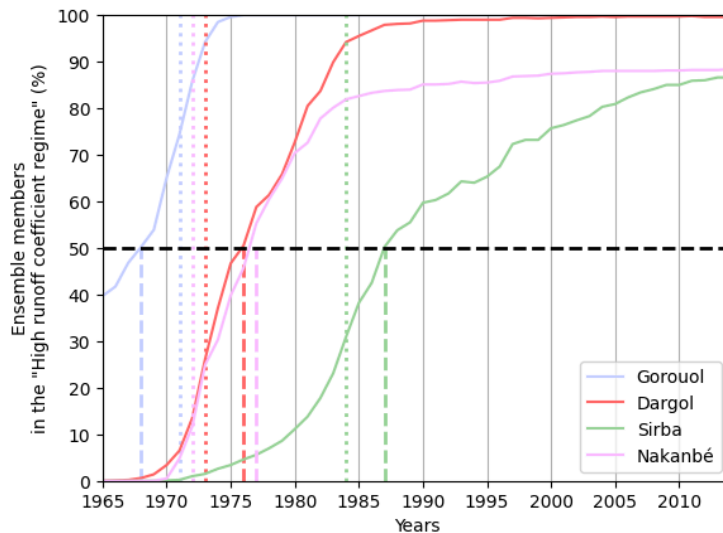

Fig 4: Updated version of Figure 7 from the manuscript, with the attractors from the submitted manuscript, to compare the old definition of "regime shift year" (vertical dashed lines) with the new definition (vertical dotted lines)

- Attractors of the revised manuscript. With the new definition of "regime shift year", we find that the shift occurred in 1972, 1973, 1983 for Nakanbe, Dargol, Sirba, respectively. For Gorouol the shift was found in 1971.
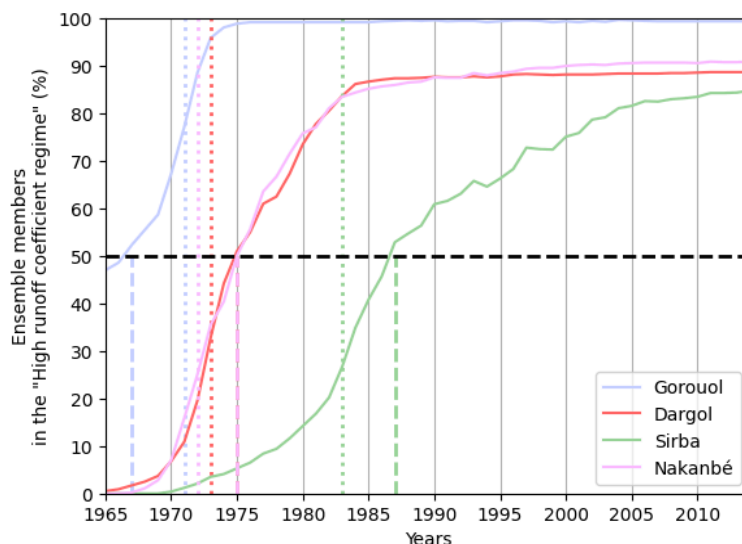

Fig 5: Updated version of Figure 7 from the manuscript, with the attractors of the revised manuscript, to compare the old definition of "regime shift year" (vertical dashed lines) with the new definition (vertical dotted lines)

Lines 200–220 (Discussion): You rightly acknowledge that the model underrepresents interannual variability and that precipitation alone is insufficient. However, this admission seems to undercut the core claim that the model can meaningfully identify regime shifts. This contradiction should be addressed more transparently. Can regime shifts truly be inferred from such a limited model?

We agree that the phrase may be misleading. We meant that the model under-represents year-to-year runoff variability, yet it well captures the decadal – low-frequency – variability, which is the time scale of both the drought signal and of the emergence of the regime shift. We will clarify this point in the revised manuscript.

Our starting assumption is that the Sahelian paradox corresponds to a regime shift. Here, we present a model that can reproduce a regime shift: we calibrate the model using observations and find that most parametrizations show a trajectory of the state variable that corresponds to a regime shift. It is likely that many other models (including much simpler models) could reproduce this regime shift. However, we think that the proposed model strikes a good balance between having some complexity and reproducing the regime shift.

Lines 205–210 (Gorouol case): The early regime shift in the Gorouol basin (before observed droughts) is indeed counterintuitive. It may reflect model artefacts from initialization, especially since 40% of ensemble members already start in the "high" regime. This undermines the claim of detecting shifts dynamically. Please explore whether this result is robust or an artefact of initial conditions.

Yes, in the manuscript, the early regime shift likely reflects either i) initialization issues and/or ii) unadapted definition of "regime shift year". Exploring other initial conditions for Gorouol seem out of reach in terms of computation, as for a given initialization the whole workflow (solving the trajectory for 1000000 sampled parameterization, selection of the ensemble with 1000 parameterizations, estimating the attractors for the whole ensemble) takes about a week on a parallelized cluster. This is why methodologically we did the most parsimonious/simple choice of initialization: the initial state value S is computed with the first observed runoff coefficient and Eq. 1. In the revised manuscript, to fix this Gorouol case, we will follow your advice from an earlier comment and adapt the definition of "regime shift year" so that it does not account for ensemble members that already start in the "high" regime.

In practice, the early shift in the Gorouol watershed is consistent with field observations, which suggest that the most sensitive areas (shallow sandy soil over rocky/clayey substratum) have been eroded very soon after the beginning of the drought, in the early 70s, whereas it occurred later on the other watersheds, see Nguyen (2015). These details will be added to the revised manuscript.

Lines 215–220: The interpretation of monostable vs. bistable ensemble members is important, but underdeveloped. If 10% of simulations do not undergo regime shifts, does this reflect real watershed variability or model limitations? Some exploration of this heterogeneity would enrich the discussion.

In the submitted manuscript Gorouol, Dargol and Sirba had only bistable ensemble members, while Nakanbé had 10% of monostable ensemble members. With the novel definition of attractors, this ratio is 6.4 %, and monostable members (4.4% and 8.1%) are also detected for Sirba and Dargol, respectively. For Gorouol, this percentage equals 0%.

We hypothesize that this difference is due to the fact that our old definition of attractors may have been wrongly detecting some bistability, e.g. upper and lower attractors that were estimated as different even though in practice they corresponded to the same attractor.

A monostable ensemble member implies that the watershed did not undergo a regime shift. As stated in the manuscript, this result first shows that the proposed model can simulate changes in runoff coefficients with or without regime shift. However, monostable members represent less than 10 % of the ensemble . This result could be related to real watershed variability, but is indeed more likely related to the main model limitations (Land cover changes and daily rainfall intensity not accounted for). .

A minor and general comment: the writing is generally clear, but at times overly dense with jargon. Consider simplifying key explanations, especially around dynamical systems concepts, to enhance accessibility for a broader hydrological audience. Also, Figures (in general) are informative, though Figures 4 and 6 could benefit from clearer legends and a brief description of axis choices (e.g. why is S bounded between 0–0.7?).

In the revised manuscript, following an early comment, we simplify the first paragraph of the introduction and explain the term "attraction basin". Before this preprint, this manuscript already underwent several rounds of revisions to ensure that it is accessible for a hydrological audience: many co-authors, with hydrological backgrounds, provided feedback in order to help the readability around dynamical systems concepts. Please do not hesitate to pinpoint us sentences where concepts remain unclear, we will try to fix that. Finally, in the revised manuscript, we will add such a brief description of axis choices for Figures 4 and 6.

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