

Dear Editor,

We are submitting a revised version of the manuscript. We first proceeded to all changes we proposed in our response to reviewers (sometimes slightly modified), and then accounted for your comments, which leads to substantial modifications, notably in the discussion.

The main changes made in the manuscript are listed below :

- Abstract: modifications to make clearer the value proposition from the start.
- Introduction: no substantial changes.
- Material and Methods: modification of the first sections to present distinctively the model, the agro-pedo-climatic context and the study site, and the model instantiation for the support site. We added a table that summarizes the context of the study, and one figure to illustrate the creation of a reservoir network.
- Results: we added a subsection presenting maps of low flow and showing the interest of the new indicator of proportion of network in low flow compared to a single observation at the outlet. The summary table of impacts and factors roles was moved from the discussion to the results.
- Discussion: modification of the structure and the idea presented, with a greater focus on the effect of the characteristics of the reservoir network and the generalization of results.

Below are our answer to your comments and to the referee comments, that we modified accordingly to your demand. Our answers are in blue. The changes that we made are in red.

Finally, we would like to thank you and the three reviewers for all your comments on our article. They have helped us to improve its clarity and to reframe it for a broader audience.

Best regards,

Henri Lechevallier on behalf of co-authors.

## Answer to editor's comments

The paper has been reviewed by three individuals with a good depth of knowledge and bringing different perspectives. The reviewers concur that the manuscript is valuable and should be published with minor revisions.

Broadly, I agree with this assessment, but I do have some aspects that I would like to see given more focus. These are in two broad categories, as listed below. Please give due consideration to the reviewer comments and suggestions, in addition to the below.

### 1. Model assumptions

While supportive overall, the reviewers questioned several of the model assumptions. Reading the author responses, I feel several should be reconsidered. In some cases the response is still somewhat unclear, such as the response regarding the dead storage assumption for reviewer 1 (seems circular--can you rephrase?). In other cases, the arguments seem weak, such as the evaporation response for reviewer 1, which needs further justification and possibly a sensitivity analysis.

Thank you for considering our response to reviewers. We clarified our responses to reviewers below for these two points. For the dead storage assumption, we also improved its presentation in the manuscript.

Assumptions are necessary for this type of modelling--that is not in question here. However, providing additional information and justification of those assumptions will help future studies to make their own decisions. I suggest that you review all cases where model assumptions are questioned, with a particular focus on cases where more than one reviewer has raised the same issue.

We totally agree that modeling assumptions should be clearly described and justified. Our model, its implementation, calibration and validation were performed in a previous work and presented in a dedicated publication (Lebon et al., 2022). We invite readers who search for a full description of the model and its parametrization to read this publication. In the manuscript, we tried to focus on presenting the specificities of the model and the representation of key processes related to small reservoirs exclusively.

We proceeded to changes to make the separation between the context of the study and the modeling assumptions to represent the management of reservoirs clearer. Section 2.2.1 presents the agro-pedo-climatic context of the study and section 2.2.2 presents the instantiation of the model for the support site.

Below are the assumptions that were questioned by the reviewers and a more detailed answer :

1. The choice of a method to assess evaporation on reservoirs (see our response to reviewer#1).
2. The leakage through dam walls and to groundwater : there is little study on reservoir leakage in the study site. In the study site, soil and subsoil are composed of fine textured elements. Compared to other contexts (e.g. presence of fractured rock, sandy soils), we can assume that the losses to groundwater are in the lower range. Given that the reservoirs are built to store water, we assume that they all have impervious beds, thus preventing infiltration. **This assumption is presented in line 113.**
3. The choice not to represent urban surfaces : the model is intended to be used in rural areas with limited urban impervious surfaces, that are assimilated to natural surfaces. **This scope is presented in lines 92 and 101.**
4. The stop of withdrawals below a certain volume : this is the implementation of a common practice in southwestern France and not a model assumption. Farmers will not empty their

reservoirs to preserve the pumping material and the quality of the irrigated water. The exact “dead volume” is specific to each farmer and can possibly vary between years. Therefore, we fixed the threshold volume at  $\frac{1}{4}$  of total capacity based on expert knowledge of regional practices. In the numerical experiment, reservoirs were dimensioned to cover a given proportion (50% and 100%) of the mean inter-annual water demand by crops. The capacity of reservoirs were then scaled considering the dead volume so that the usable water corresponds to the calculated proportion. Therefore, the exact value for the dead-volume is not expected to considerably affect our results (as long as there is a dead volume). **This practice is presented in line 143, and it’s implementation in the model in line 161.**

## 2. Framing the paper

This is a single catchment modelling study, which normally would not be sufficient for publication in a journal like HESS. This is because the results are not generalisable when only one catchment is considered. However, the value is still clear because new aspects are being considered, such as the impacts spatially across the stream network and the hydrological impact of return flows. Given this framing, I suggest:

Firstly, please review the manuscript to make clearer both the value proposition and the limitations. For example, the abstract needs to mention the limitations inherent in a single catchment study and it needs to be clearer on the value proposition as stated above. Please review the main text also in this regard.

We think that our numerical experiment and our result are interesting for the scientific community in general, and we appreciate your feedback that stresses the need to make our value proposition clearer. Although our method builds on numerical simulations for one single catchment, our objective is not to assess the impacts of reservoirs for the current network or some scenarios on plausible developments of the network, but to gain knowledge on the factors of impacts of a reservoir network that would likely be the same in other contexts similar to ours. In the first version of the article, we partly omit to identify these “other contexts” in which our analysis remains valid, a point that we corrected in this new version.

We think that our study is innovative in different aspects summarized as follow:

- The use of a new and innovative methodological approach that consists in simulating 90 alternative hypothetical reservoir networks with a spatially distributed model that considers the time and space variability of withdrawals in reservoirs.
- The quantification of impacts not only at the outlet, but considering the low flow experienced along the entire hydrological network.
- The evaluation and the interpretation of the influence of relatively unexplored characteristics of the reservoir network, especially the distribution of reservoirs along the stream.
- The consideration of the interannual and seasonal variability of impacts as a consequence of meteorological variability.

Here are the main changes that we made to make this value proposition clearer:

- **Abstract:** less emphasis is given on our results, we now announce that the catchment is representative of a given context that will be important to understand the results (“The catchment is representative of the agro-pedo-climatic context of southwestern France characterized by the presence of multiple small farm reservoirs for irrigating field crops” L8), and raise the attention on the limits of a single-catchment experiment by indicating that the generality of the results will be questioned (“and question the conditions under which

this interpretation can be generalized to other contexts” L20). Each of the four innovative aspects presented above are now mentioned in the abstract.

- Results: the results were not considerably modified, but we added a new section that presents the new indicator of low flow (section 3.1).
- Discussion: the discussion was thoroughly modified and is now build around the four innovative aspects (more details in our response to your next commentary).
- Conclusion: the conclusion already reported the innovative aspects in the form of key lessons. We made small modifications to expose the question of generalization.

Secondly, please alter the results and discussion sections so that it has more emphasis on explaining the concepts--concepts being explored for the first time--and less emphasis on the results for this particular catchment. You have already started doing this in your response to referee 2 at line 298, please do more of this. By explaining the concepts and showing how they apply in this case, your aim should be to prompt future researchers or catchment managers to ask similar questions of their catchments.

In the result section, the analysis of the numerical experiment is carried out step by step:

- Section 3.2 (formerly 3.1) presents the impacts of small reservoirs as would any study on the topic do, with the particularity of showing the variability between simulations.
- The natural follow-up to this section consists in trying to explain this variability with the study factor. Therefore, section 3.3 (formerly 3.2) analyses the direction and the magnitude of the influence of each factor on a selection of relevant indicators.
- Finally, section 3.4 (formerly 3.3) tries to clarify what processes differ and explain the variability of impacts between the different simulations. We assume (and show) that the amount of withdrawals and the timing of reservoir refill are two important variables to consider in this analysis.

For us, this construction for the result section is the most telling. We decided not to change it (except for the changes proposed in our initial responses to reviewers). **As an introduction to our results, we added a small section, independent of the three other sections, that presents our indicator of low-flow with maps.**

**The structure of the discussion was entirely changed. Some content has been removed, notably the summary of results for our catchment, and some added.** The discussion is now structured as follows:

- Its introduction underlines the originality of the work, discuss the realism of the randomly generated networks, reminds that our work is a single-catchment experiment, and precise the context and its representativity.
- Section 4.1 “How do the characteristics of the reservoir network affect flows?” addresses the main question of the article. First, we provide an interpretation of factors roles in our case building on the processes behind the impacts of reservoirs, especially the timing, amount, and location of withdrawals (4.1.1). These processes are likely to be the same in other catchments. Second, we come back on the specificities of our context and discuss the conditions to generalize our results to other contexts (4.1.2). Third, we comment the high residuals of the ANOVAs that show that part of the variability of impacts is not captured by the three characteristics of the reservoir network studied (4.1.3).
- Section 4.2 “Methodological advances” comes back to methodological aspects of the study of the impacts of reservoirs, that are relevant in general: the use of a new indicator for low-flow, the handling of the inter-and subannual variabilities.

To me, a clear strategy to achieve this would be to use maps more to show the concepts spatially--indeed, it is rather incongruous that a paper that is framed spatially (“we still have little knowledge

of the spatial factors") has no maps in the results section! Perhaps some sequences of maps showing the seasonal evolution of low flows and sensitivity to dam locations?

The wording of the first manuscript submitted caused confusion. We analyze the effects of reservoir network characteristics, particularly its spatial organization, on watershed hydrology. The simulated flows, which form the basis for calculating impact indicators, are sensitive to this spatial organization. However, we have chosen to calculate impact indicators that reflect and integrate intra-basin variability without being spatialized in their final expression. This explains why we did not initially produce a map of indicators. We rephrased one sentence in the abstract and one in the introduction that were misleading:

- “However, we still have little knowledge of the spatial factors that drive these cumulative impacts. In this work, the effects of the distribution of small reservoirs in a catchment on their hydrological impacts are investigated with a modeling approach” L4 was changed to: “However, the influence of the composition and spatial configuration of a reservoir network on its hydrological impacts is still largely unknown. In this work, we investigate the influence of various characteristics of a reservoir network with a modeling approach.” L4.
- “For low flows, the cumulative impact was quantified and analyzed spatially with respect to the stream network rather than solely at the catchment scale” L78 was changed to: “The magnitude of low flow experienced along the stream was summarized in one indicator that reflects the state of the entire network rather than solely at the outlet” 81.

In the paper, we present a new and original numerical experiment and analyze the results of this experiment. In the analysis, we chose to use synthetic indicators on which ANOVA’s could be performed, and extracted as much information as possible from this analysis. At first, we excluded working with maps, an approach that we want to develop in the future, but with an experiment based on less simulations. To address your commentary, we decided to include a new section (section 3.1) in the results that shows maps for four simulations. This section has two objectives: (i) illustrate how the new indicator of low flow relates to the low-flow experienced along the stream, and (ii) present a nice perspective on the potential of maps to study the cumulative impacts of reservoirs on low-flow. We hope that readers will understand our reasons for not carrying this analysis further in the article.

Likewise, a map or schematic expressing the concept of return flows would be valuable (noting that one of the reviewers was not familiar with the concept, thus you can expect many of your readers will also be unfamiliar).

Irrigation return flow is a well-documented phenomenon in irrigated catchments. To avoid overburdening the manuscript with additional schematics or figures, we have opted to include a concise explanation (L441) and direct readers to Ketchum et al. (2023) for a comprehensive description and illustration of the return flow process.

Lastly, please give more consideration to the specifics of your case study and consider how results might differ elsewhere. For example, the irrigation here is not universal; many farm reservoirs support stock and/or domestic use only. Do you expect your results to hold up in this case? Likewise, the climate here is somewhat Mediterranean; what if it were clearly summer dominant rainfall instead? etc. Please pull from the literature to understand the breadth of different conditions and modes of usage that are faced by different farm reservoirs modellers around the world.

Thank you for pointing this. In the paper, we insisted in the material and method and in the beginning of the discussion to present our context and give precisions on the generality of the results (section 2.2.1 and lines 429-434 in the first preprint). In the revised version, we tried to clarify our context and the underlining modeling assumptions even more: we split section 2.2.1

into 2.2.1 and 2.2.2 to present in two times the context and study site and the instantiation of the model. We added a new table (Table 1), that summarizes the agro-pedo-climatic context of the study. The discussion opens on a reflection and the representativity of the context (Lines 484-488). Then, we dedicate section 4.1.2 to the question of the generalization of results. We argue that, under some conditions that we indicate, our results could still be valid in other contexts, with local specificities. We then discuss the specificities of our case. We choose not to make speculations on the generalization of our results to entirely different contexts (e.g. stock reservoir, different hydrological regime), as we believe that it requires specific works.

Thank you for a valuable and high quality study. I look forward to receiving a revised manuscript in due course. The study will be passed back to the reviewers for another round of review. Feel free to ask journal staff for extra time if you need it, to respond to the above comments.

Thank you for your considerable feedback that helped us increase the quality of our work.

## Answer to Referee #1 :

This manuscript describes the cumulative impact of small reservoirs on the hydrology of a small catchment area in south-western France. These reservoirs are generated randomly and are connected to the river network. Their characteristics include their water storage capacity, their spatial distribution across the catchment area and their number. The metrics used to assess the impact of these different components are annual flows, summer flows, and the proportion of the hydrographic network at low flow each year. The study is based on 20-year numerical simulations. It shows that the impact of reservoirs is cumulative along the river network and assesses the effects of the distribution, number and capacity of reservoirs on flows, and studies the associated processes. This work is very interesting and also sensitive because it attempts to provide answers to societal questions, in particular the storage of water for irrigation in dedicated reservoirs.

I find this article very well structured and well written. The conclusions are based on objective evidence and clear figures, and the limitations of the study are well presented in the discussion. I recommend publication of this article after minor revisions.

Thank you for the detailed review of our manuscript and your positive feedback that helped us in the revision of our paper. We hope our answer will help clarify some points.

### Comments:

- L109: evaporation from the reservoir is considered to be 60% of the reference evaporation, which I assume corresponds to potential evapotranspiration. How valid is this approximation and what is the associated uncertainty? It is important to give an order of magnitude for annual evaporation, as well as annual withdrawals, as these contribute to direct mass loss.

The relationship between reference evapotranspiration (in our case obtained with the Penman-Monteith equation) and pond evaporation that we use in MHYDAS-small-reservoir is purely empirical. The value of 0.6 was taken as a common value as it is used from the reservoir and pond modules of SWAT (Neitsch et al., 2011).

This kind of relationship is used in many models representing small reservoirs. However, only few authors give their coefficient. On more than thirty approaches to model small farm reservoirs in hydrological models that we are aware of, only 4 give the used value for this parameter (note that all of these 30+ approaches do not necessarily model evaporation or use the same method as us). Jayatilaka et al. (2003) use a value of 0.8 in the model CASCADE, Peter et al. (2014) a value of 0.7 in ResNetM, McMurray (2006) a value of 0.75 (in winter only), and Rabelo (2021) a value of 1.0 in a modified version of SWAT. Note that the three first use the A-pan evapotranspiration as reference.

We see that values vary from one study to another, which makes sense since the climatic context is different. Only, there is little knowledge basis on which to choose an appropriate value. In Southwestern France, there is no study allowing us to justify the choice of one or another value. In a Mediterranean context (with a more important evaporative demand), Martinez-Alvarez et al. (2007) used a modeling approach (energy balance) to derive relationships between A-pan evapotranspiration and evaporation from irrigation reservoirs. They found that the pan-coefficient, as they call it, varies thorough the year, with the local weather conditions, and the dimensions of reservoirs. Values are ranging from 0.5-0.6 to 1.2, and are mostly comprised between 0.8 and 1.1. These values are proposed with the A-pan evaporation as reference. Since the A-pan evaporation is typically higher than the Penman-Monteith evapotranspiration (Allen et al., 1998, Chapter 4),

coefficients between Penman-Monteith evapotranspiration and actual evaporation close to or higher than 1 would be justified in this context.

The choice of 0.6 for this coefficient was a pragmatical first choice in the absence of more elements to choose a value in our context. Given this analysis, 0.6 is in the lower range of plausible values, and the representation of evaporation would certainly require reevaluation if the model is to be used to assess real-life scenarios.

The comparison of evaporation to withdrawals in the model is presented in the supplementary material. For the revised version of the article, we decided to improve this additional figure and present the interannual and subannual variability of evaporation and withdrawals (Figure S4). This figure reveals that:

- The variability of evaporation between simulations with the same capacity is usually low.
- In summer especially, the variability of evaporation is small compared to the variability of withdrawals in situations with equal storage capacity.

Therefore, we can expect that the spatial configuration of reservoirs (e.g. number and distribution) has limited effect on the evaporation, justifying that we focus on withdrawals and not on evaporation. Note that these observations also result from the choice of standardized shapes for reservoirs. In our case, associating the same shape to all reservoirs reduces the number of factors of variability studied.

It would be interesting to perform an analysis of the sensitivity of results to the value of the evaporation coefficient as proposed by the editor, but this is out of the scope of the article. Still, we can make few assumptions on the possible effects of an increase in evaporation:

- In spring, reservoirs are usually full, and we can expect that an increase in the evaporation coefficient will result in an increase in evaporation of the same magnitude for all tested networks.
- In summer, enhanced evaporation losses would have two implications: (i) reservoirs will be emptied faster, reducing the amount of withdrawals, and (ii) once reservoirs reach  $\frac{1}{4}$  of their capacity and withdrawals stop, the evaporation losses will be more important, delaying the recharge in autumn (or winter) and eventually limiting the summer recharge which enabled higher withdrawals in situations where the stored volume is a limiting factor. For point (i), withdrawals are currently at least 4 times higher than evaporation losses. Even considering higher evaporation losses, the withdrawals would still remain the main loss term on reservoirs.

- L117: withdrawals are possible if the volume of water is greater than  $\frac{1}{4}$  of the reservoir's capacity: what average water depth does this correspond to, given that the shape is an inverted pyramid? Is this compatible with the characteristics of the withdrawal pumps?

In the region, farmers stop pumping before reservoirs dry to preserve the pumping material and the quality of irrigated water. As each farmer has its own threshold, depending on the reservoir and possibly the year, we have to make an assumption in the model to consider this common practice. For example, we could set a threshold on the reservoir depth or the reservoir volume. In our case, we set a threshold at  $\frac{1}{4}$  of reservoir capacity based on expert knowledge of regional practice.

In our case, this means that, for a given total storage capacity on the catchment, the total usable water storage is always  $\frac{3}{4}$  of total storage capacity, irrespective of the number of reservoirs, meaning that this dead volume will not be a strong factor of variability between simulations.

In the paper, we now better explain this practice, its representation in the model, and we discuss its influence on results:

- L143 : “In southwestern France, farmers usually stop pumping in reservoirs before they dry to preserve the pumping material and ensure the quality of the irrigation water.”
- L161: “there is no restriction on annual withdrawals, but there is a threshold volume below which withdrawals are not allowed to represent this common practice. We set this threshold at  $\frac{1}{4}$  of the reservoir’s capacity (as in Lardy et al., 2016).”
- L555: “The dead volume practice implies that there are no withdrawals when reservoirs reach  $\frac{1}{4}$  of their capacity, but that evaporation can continue. For the same utilizable...”

- L114: on the map of France in Figure 1, there are white pixels in the Rhone valley that should not be there. What do they correspond to?

Thank you for this pointing this out. This is an oversight on our part. This has been corrected in the new version of the article.

- L127: average annual rainfall is 675 mm: how was this calculated? Using SAFRAN data or measurements from rain gauges located in the catchment area?

This was calculated using SAFRAN data. As part of our field instrumentation on the Gélon catchment, we have a single rain gauge located at the outlet of the catchment, but we do not have enough record yet ( $< 10$  years). For the years with available data, there is a good concordance between the measured data and the SAFRAN predictions.

- L128: The total volume capacity of the reservoirs in the basin is estimated at 205,000 m<sup>3</sup>. Is this estimate based on the pyramidal shape of the reservoirs or does it come from data describing the various structures?

The data on small reservoirs were obtained from a previous work in a larger area conducted by another laboratory. This study combined multiple databases from various organizations to collect as much information as possible on small reservoirs in the area. Therefore, the data were collected using different methodologies, including field surveys and satellite imagery. For the Gélon catchment, we performed a validation on the field for the 25 reservoirs. As some values of reservoir capacity were obviously incorrect in the database, these were coarsely estimated on the field and corrected.

- L140: What do you mean by “better match expectations”?

In the model used by Lebon et al. (2022), groundwater units were only drained at confluences between two portions of the hydrological network, meaning that the stream discharge at the different network heads was only constituted of surface runoff water. In this context of numerous hill aquifers, aquifers are drained all along the hydrological network and not only at confluences. The new groundwater units now better represent this continuous groundwater discharge. That is what we meant with “better match expectations”.

To clarify this point, we changed “This adjustment did not considerably modify the flows at the outlet, but the discharges in the hydrological network better matched expectations” to “This adjustment did not considerably modify the flows at the outlet, but the new groundwater units better represent a continuous water supply from shallow hill aquifers along the hydrological network” (L154).

- L147: An example of one or two reservoir distributions would be helpful. For example, showing one distribution of the 7, 14 or 21 downstream reservoirs with the two capacities (two colours) as in Figure 1 and the main irrigable crop would help to clarify the ideas.

Thank you for this commentary. We showed examples of reservoir distributions in the supplementary material, but it seems important for the understanding of the article to place them in the main manuscript.

We therefore moved figure S2 of the previous Supplementary material to the main text (Figure 2). It shows the creation of a reservoir network and the selection of nearby irrigated parcels. Furthermore, on the demand of the editor, we provide a new Figure in the results section showing maps for 3 different reservoir networks (Figure 4).

- L180: what is the proportion of reservoirs not connected to the hydrographic network compared to those that are connected? They also contribute to water storage for irrigation.

In the Gélon catchment, 9 reservoirs are not connected to the hydrological network. They have been removed in the numerical experiment as explained in 2.2.4. The addition of the new figures will also clarify this point.

- L225: a warm-up period of 5 years is considered: does this mean that the simulated data used start in September 2000? Or is the warm-up from 1990 to 1995? Please clarify.

25 years are simulated, and among these 25 years, only 20 can be analyzed. The analysis can start on 2000/09/01. Note that in the result section, only 19 years are analyzed, starting on 2001/04/01 as it was more relevant to study the impact of reservoirs on a period where they are first emptied and then refilled.

We changed “These five years are not included in the analysis” to : “These five years are not included in the analysis. Therefore, the analysis can start in September 2000.”

Note that in the first version of the article, we precise few lines later (L260, L281 of the new version): “The simulation results are thus analyzed from 2001/04/01 to 2020/03/31, which constitutes a total of 19 years”.

- L233: references to Vidal et al. (2010) and Le Moigne et al. (2020) can be added for SAFRAN. Can you clarify if the reference evapotranspiration is computed from SAFRAN data and at what frequency? Same question for min and max temperatures, do they come from the SAFRAN reanalysis?

For Vidal et al. (2010), we found a reference with this doi : 10.1002/joc.2003. This seems indeed to be an important reference that we will add into the paper (see below our proposed reformulation).

For Le Moigne et al. (2020), we found a reference with this doi : <https://doi.org/10.5194/gmd-13-3925-2020>. Though the authors use SAFRAN as a component of their hydrometeorological model,

we don't find that it brings additional information on the SAFRAN dataset that are relevant in our study. We decided not to include it.

Thank you to ask about the exact data that we use as inputs in the model, as it appears that we made a mistake in the first version of the article. The data was obtained in mid-summer 2024 with the latest reanalysis. Here are the exact product that we used for each input variable :

Rainfall : hourly values (column Rainf).

T\_moy : hourly values (column Tair).

T\_min : daily values (column tinf\_h\_q).

ET0 : daily values (column etp\_q, which corresponds to potential evaporation computed with the Penman-Monteith equation).

We propose a new formulation for this paragraph (see our answer to your commentary below).

- L236: You state that meteorological data variability is taken into account: I would temper this statement, as the climate in such a small area, covered by four contiguous points, probably does not vary greatly, at least you have not demonstrated that it does. I suggest removing this idea of variability. However, it is interesting to note that Gélon covers only four cells of the SAFRAN grid. Further on, you only take one cell (8558) into account in your comparison. You could at least show that the annual precipitation for these four grid points is very similar, which would allow you to take only one into account.

Thank you for drawing our attention to this point. The statement has been removed. First, there is a mistake, the Gélon only intersects two cells (which has been corrected). Second, more than 97% of the catchment is located on the same cell, that's why we only presented this cell (see Figure 43 in Lebon 2021).

We propose to changed this paragraph to : "The weather data are composed of hourly SAFRAN data for rainfall and temperature, and daily data for the minimum daily temperature and reference evapotranspiration (Penman-Monteith). The SAFRAN climatic data are provided by Météo-France and were downloaded via the SICLIMA platform developed by AgroClim-INRAE. The data are reanalysis of observed data on 8x8 km<sup>2</sup> cells (Bertuzzi et al., 2022; Vidal et al., 2010). The Gélon catchment intersects with 2 of these cells. One of these two covers more than 97 % of the catchment area. The seasonal rainfall for the main cell is presented in Figure 3." (L254)

- L252: Figure 2, Medians are extending before 2001 and after 2019: do they use data covering 2000-2020? If not lines must be cropped to adjust to 2001-2019.

Medians were computed for the presented period, which will be the period of analysis. This has been done.

- L300: the sentence 'highlights the role of weather' is not very adapted and accurate: please rephrase

True, we changed it for : "which shows that the weather is a determinant factor of reservoir impacts". (L334)

- L321: (i) and (v) are the same, (v) is the autumn proportion of the framework in low flow

OK, thanks !

- L327: Fig 5 (a) and (d) respectively

OK, thanks !

- L329: 'the boxes are more separated' is not very adapted, perhaps change it into 'each year, the departure to the median is systematically more pronounced in (a) as compared to (d)'

We propose to change it for: “each year, the intersection between boxes is systematically smaller in (a) than in (d).” (L364)

- L403: equation should be  $y=-(3/4)x$  ; in Figure 7 equations should contain brackets  $y=-(a/b)x$

OK, this was changed.

Edits:

- L222: parameterization OK, thanks !

- L166: reservoir OK, thanks !

- L176:  $1.06 \text{ km}^{-2}$  OK, thanks !

- L250: reference simulation

In the paper, we frequently use the reference situation to designate the scenario without reservoirs, or to designate the simulation of this reference situation. As we never use the term “reference simulation”, we think it would be clearer to keep “reference situation” in L250.

## Answer to Referee #2 :

The article presents an analysis on impacts of the variability of small reservoirs networks configuration in terms of their number, storage capacity and distribution in space. As the authors state, this type of investigation is a new addition to the study of small reservoirs impacts on the water cycle. The article is well written, and the authors described with clarity and detail the configuration tested and the outcomes they achieved. Below I've listed some comments to the authors, mainly clarification of some parts, then some edits to the text.

I suggest the article to be published after minor revision addressing the comments below.

Thank you for the positive appreciation of our work and your many suggestions that helped us in the revision of our article.

### Comments/revisions

Line 94: only agricultural and natural surfaces are referenced, is the model not able to represent urban surfaces, or was this a deliberate choice? This could be addressed in the text

Currently, the model cannot represent urban surfaces. In the text, we added two sentences to address this question: L92 "It [the model] is adapted for agricultural catchments that are sparsely urbanized", and L101 "Urban areas are assimilated to natural surfaces in the model, as they usually cover a small fraction of agricultural catchments."

Line 109-110: I think a small justification for not considering percolation from the bed and walls could be provided here. The authors for example state in another part that the soil is mostly impermeable, this could be an explanation but it is not explicit.

Small reservoirs are built to store water for irrigation and to limit water losses by percolation. Therefore, we can reasonably assume that the reservoir beds are impervious, thus preventing infiltration. In the reality, some of the reservoirs may leak but it would be difficult to get any estimation of this term.

This assumption is now made explicitly in line 113: "Percolation from the reservoir bed to groundwater or through the dam wall is not considered in the model, as we assume that irrigation reservoirs have impervious bed to prevent percolation."

Line 117: is this decision backed by some prior knowledge e.g. based on small reservoirs functionality?

This decision is the representation of a common practice in southwestern France. Farmers stop pumping before reservoirs dry to preserve the pumping material and the quality of irrigated water.

The value of  $\frac{1}{4}$  is a model assumption based on expert knowledge on the regional practices.

In the paper, we now better explain this practice, its representation in the model, and we discuss its influence on results:

- L143 : "In southwestern France, farmers usually stop pumping in reservoirs before they dry to preserve the pumping material and ensure the quality of the irrigation water."
- L161: "there is no restriction on annual withdrawals, but there is a threshold volume below which withdrawals are not allowed to represent this common practice. We set this threshold at  $\frac{1}{4}$  of the reservoir's capacity (as in Lardy et al., 2016)."

- L555: “The dead volume practice implies that there are no withdrawals when reservoirs reach 1/4 of their capacity, but that evaporation can continue. For the same utilizable...”

Line 179-180: the authors could provide more details explaining what are the "specific locations" of the reservoirs

Here, “specific locations” means that hill reservoirs are usually found in locations that are not fully random, but that could be considered as “strategic” in terms of drained area and the surface runoff generated within it. Field observations also indicates that these reservoirs are often positioned to intercept subsurface flow.

In the revised article, we changed “As hill reservoirs are usually found in specific locations, a random placement does not make much sense for this type of reservoir” (L180) to “As hill reservoirs are usually found in locations where surface and subsurface flow converge, which is not captured by the model, a random placement would not be meaningful for this type of reservoir.” (L197)

Line 180: at Line 96, the hydrological network is defined as the RSs. In Figure 1, REs are not all located on RSs. This is in conflict with the sentence at line 180-1.

Figure 1 shows the current distribution of reservoirs in the Gélon catchment, with both hill reservoirs and small dams. In the numerical experiment, we remove all reservoirs before generating a new reservoir network with reservoirs connected to the stream only.

To improve the clarity of the presentation of the numerical experiment, we moved figure S2 from the supplementary material to the main text (Figure S2).

Line 221: the authors could state here at what time steps the simulations are performed

In line 92, we precise that the model runs at an hourly time step for water routing and a daily time step for crop growth. According to us, it is not necessary to repeat it in line 221.

Line 298: the authors introduce here "irrigation return flows", which are later addressed in a dedicated section, but it is not clear to me what these flows are or why they happen, are they a component of the model? How and why does it return to the hydrological network? The reservoirs are emptied at the end of the irrigation season? Is this happening in reality?

Irrigation water entering the soil increases soil water content. Soil water can either (i) be transpired by plants, (ii) evaporate to the atmosphere, (iii) percolate to groundwater. At harvest, not all irrigation water has necessarily been used by plants or evaporated. Some of it either leached to groundwater, or still remains in the soil, and will eventually percolate later once the soil reaches saturation after subsequent rain events. These percolation fluxes that would not happen without irrigation increase groundwater stocks and therefore streamflows. Such additional flows, generated by irrigation, are referred to as irrigation return flows (Ketchum et al., 2023).

For more consistency in the result section, we proceeded to some changes :

- Removal of the first mention to irrigation return flow in L298 (first version of the article). This paragraph (former 3.1.2, now 3.2.2) aims at describing the observed impacts. The statement on irrigation return flow is an interpretation and does not belong here.

- Definition of the irrigation return flows in the dedicated section (former 3.3.1, now 4.3.1 “Withdrawals volumes and irrigation return flows”): “Thus, on average, 3/4 of the irrigation water is used by the plants or evaporated. The remaining 1/4 returned to the hydrological network as irrigation return flow, defined as the portion of irrigation water that flows back to the hydrological network (Ketchum et al., 2023). Irrigation increases soil water content. The applied water can therefore contribute to different fluxes, i.e. (i) crop transpiration, (ii) soil evaporation, and (iii) percolation. Irrigation return flows occur when part of the irrigated water percolates to groundwater, which increases water table levels and streamflows. Since percolation only occurs when soils water content is above field capacity, irrigation return flows can be delayed compared to the irrigation period. The timing and amount of irrigation return flow can be critical for understanding the effects of small reservoirs, not only on outlet discharges, but also on low flows. These return flows can explain why, for some years, the autumnal outlet discharge increases compared to the reference situation (e.g. 2002, 2005, 2006, 2011, 2017). These return flows occur at reach sections that are located near irrigated fields, and can locally sustain flows during dry period. That explains why the proportion of network in low-flow decreases for some years, especially in autumn (e.g. 2001, 2016, 2017). To explain the variability observed ...” (L440)

Line 391-392: withdrawals are based on the decision model. Can observation (i) be proposed as an "absolute" observation as it is proposed here?

This statement is a commentary on Figure 9 (formerly 7) to describe the obtained results. It is therefore not an absolute observation and it is not presented as such. In our case, this is an expected result since reservoirs are meant to be intensively used and the lowest value of capacity was chosen to be insufficient to cover all water demand.

In a real case, we can generally expect that an increase in storage capacity in a catchment will be linked to an increase in water use, provided that reservoirs can be fully filled during the wet season. This can vary depending on the context (e.g. are reservoirs also used for domestic purposes? Are the reservoirs meant to cover the demand for pluri-annual drought?). In the revised version of the article, we exposed the study context and the modeling assumptions on reservoir management more clearly (sections 2.2.1 and 2.2.2). The discussion now includes a reflection on the generalization of our results (sections 4.1.1 and 4.1.2).

## Edits

Line 13: here, “low flow proportions” is not too clear, I would change to “the proportion of low flow”

Ok, this has been done.

Line 14: would change to "For the two indicators", since they are presented a few sentences before

This sentence has been removed.

Line 43-44: reference to Colombo et al., 2024 can be done as a more recent example of small reservoirs cumulative impacts study through modeling (<https://doi.org/10.1016/j.jhydrol.2024.130640>)

In this sentence, we refer to Habets et al. (2018) as this reference presents the variety of existing models, including WASA (see Table 1). The reference to Colombo et al. (2024) was added in our

discussion: “A possible approach would be to build on the concept of downstreamness (Colombo et al., 2024)” (L604).

Line 78: streamflow

Ok, this has been changed.

Line 276: i would suggest to put (no reservoirs) after "reference situation"

Ok, this has been changed.

Line 418: I would remove "figure not shown"

Ok, this has been changed.

Table 2 caption: where the upward and downward arrows are indicated I would add text stating something like "increase" and "decrease", as it could be misunderstood as "positive" and "negative"

Ok, this has been added.

### **Answer to Referee #3 :**

This manuscript examines the impact of small dams on the flow regime in a small catchment in south western France. The aim of the study appears to be to systematically assess how the spatial arrangements and physical characteristics of dams affect their hydrological impact.

The study uses a series of randomly generated arrangements of dams placed on the stream network with randomly generated physical characteristics. While many studies tend to focus on annual flows, this study assesses additional flow metrics including seasonal flows and the proportion of the stream network experiencing low flows. Notably, it models the effect of irrigation return flows which is novel. This appears to have a significant effect on results.

The results highlight some interesting relationships between the hydrological impacts and the number, volume, and arrangement of dams. However, there are a small number of assumptions which limit the applicability, including the use of water for irrigation with return flows, passing environmental flows, and limiting withdrawals to the top 75% of dam capacity. Modifying these parameters (perhaps in future studies) could provide results which are relevant to many other contexts and jurisdictions.

The article is well written, easy to understand, and clearly structured. The discussion and results are relevant for the fields of water resources management and hydrology. I am keen to see this paper published, however as noted by other reviewers, it could be improved with some minor revisions.

Thank you for this positive feedback, that we considered in the revised version of the article to improve the presentation of our numerical experiment, to better explain the process of irrigation return flows, and to discuss the generality of results further.

Specific comments:

L109 – Evaporation from the surface of each dam is assumed to be equal to 60% of ET. This may be appropriate, but it would be good to see some justification for this. Other evaporation variables may be more suitable for this purpose, such as Morton's shallow lake evaporation (refer to doi 10.5194/hess-17-1331-2013).

In our work, we consider that evaporation from reservoirs can be simulated based on a proportional relation between this evaporation and the reference evapotranspiration. This relation was shown to be appropriate in previous studies (Martínez Alvarez et al., 2007). The main choice is about the coefficient of proportionality that can vary depending on the conditions. We used the value of 0.6, which is a common value, especially used in SWAT model applications for simulating actual pond evaporation. Our approach to modeling pond evaporation is empirical, and while it is widely used in hydrological models to represent small reservoir evaporation, few studies report the coefficient values used. We acknowledge that the representation of evaporation could be improved in our model, and we thank you for providing the useful reference.

L114 – In the results section, many references are made to return flows from irrigation. This feature of the model and assumptions around it are not described anywhere in the method (eg. percent of flow returned, which reach does irrigation water return to). I would expect to find this information in Section 2.1.3. I realise this information may be present in one of the references given, but it appears to be important for this study so it needs to be provided here.

This point was also raised by Reviewer #2. In our paper, we incorrectly assumed that the concept of irrigation return flows was widely understood. This term refers to the portion of irrigated water that is neither evaporated nor transpired, but instead percolates to groundwater and consequently may further re-enters the hydrological network. Our model does not assume a fixed proportion of irrigation return flows for a given irrigation height. These are computed as part of the percolation from the soil water balance in the soil-crop model. *A posteriori*, we observe that streamflow increases in late summer and autumn in situations with irrigation compared to the non-irrigated reference situation. We attribute this increase to irrigation return flows. By comparing the change in outlet discharge and the total withdrawals in reservoirs, we can estimate these return flow. If all withdrawn water for irrigation was evaporated or transpired, the net decrease in outlet discharge would equal the total withdrawals. Figure 7 of the paper shows that the net decrease ranges between  $\frac{1}{2}$  and 1 times the withdrawals, with the difference representing irrigation return flows.

For more consistency in the result section, we proceeded to some changes :

- Removal of the first mention to irrigation return flow in L298 (first version of the article). This paragraph (former 3.1.2, now 3.2.2) aims at describing the observed impacts. The statement on irrigation return flow is an interpretation and does not belong here.
- Definition of the irrigation return flows in the dedicated section (former 3.3.1, now 4.3.1 “Withdrawals volumes and irrigation return flows”): “Thus, on average,  $\frac{3}{4}$  of the irrigation water is used by the plants or evaporated. The remaining  $\frac{1}{4}$  returned to the hydrological network as irrigation return flow, defined as the portion of irrigation water that flows back to the hydrological network (Ketchum et al., 2023). Irrigation increases soil water content. The applied water can therefore contribute to different fluxes, i.e. (i) crop transpiration, (ii) soil evaporation, and (iii) percolation. Irrigation return flows occur when part of the irrigated water percolates to groundwater, which increases water table levels and streamflows. Since percolation only occurs when soils water content is above field capacity, irrigation return flows can be delayed compared to the irrigation period. The timing and amount of irrigation return flow can be critical for understanding the effects of small reservoirs, not only on outlet discharges, but also on low flows. These return flows can explain why, for some years, the autumnal outlet discharge increases compared to the reference situation (e.g. 2002, 2005, 2006, 2011, 2017). These return flows occur at reach sections that are located near irrigated fields, and can locally sustain flows during dry period. That explains why the proportion of network in low-flow decreases for some years, especially in autumn (e.g. 2001, 2016, 2017). To explain the variability observed ...” (L440)

L117 – As others have noted, assuming no withdrawals if the dams is below 25% capacity seems arbitrary. Is this a regulated limit? Or standard operating procedure? In most parts of the world, farmers will extract water from their dams until it is just a small pool of mud. A 10 word explanation is fine.

As your comment is related to a comment of Reviewer #1, we will provide the same response.

In the region, farmers stop pumping before reservoirs dry to preserve the pumping material and the quality of irrigated water. As each farmer has its own threshold, depending on the reservoir and possibly the year, we have to make an assumption in the model to consider this common practice.

For example, we could set a threshold on the reservoir depth or the reservoir volume. In our case, we set a threshold at 1/4 of reservoir capacity based on expert knowledge of regional practice.

In our case, this means that, for a given total storage capacity on the catchment, the total usable water storage is always  $\frac{3}{4}$  of total storage capacity, irrespective of the number of reservoirs, meaning that this dead volume will not be a strong factor of variability between simulations.

- In the paper, we now better explain this practice, its representation in the model, and we discuss its influence on results:
- L143 : “In southwestern France, farmers usually stop pumping in reservoirs before they dry to preserve the pumping material and ensure the quality of the irrigation water.”
- L161: “there is no restriction on annual withdrawals, but there is a threshold volume below which withdrawals are not allowed to represent this common practice. We set this threshold at 1/4 of the reservoir’s capacity (as in Lardy et al., 2016).”
- L555: “The dead volume practice implies that there are no withdrawals when reservoirs reach 1/4 of their capacity, but that evaporation can continue. For the same utilizable...”

L155 – I do not quite understand the scenarios. In particular, the phrase ‘each combination of modalities is repeated 5 times’ is not clear. Exactly what is repeated 5 times? And what is different about each of these 5 scenarios?

The key point of our numerical experiment is the random generation of reservoir networks. Our network generator requires three parameters : the number of reservoirs, the total capacity to be distributed between these reservoirs, and a parameter controlling the random spatial distribution of reservoirs (upstream vs downstream). The experimental plan consists in testing three different values for the number of reservoirs, two for the total capacity, and three for the distribution of reservoirs. Since network generation is stochastic, we performed five repetitions for each combination of parameters, resulting in a total of 90 generated networks and 90 simulations ( $3 \times 2 \times 3 \times 5$ ). For example, we have five different reservoir networks with 21 reservoirs for a total capacity of 140000 m<sup>2</sup>, and with a distribution of reservoirs classified as “upstream”.

As the network generation process was unclear to another other reviewer also, we moved figure S2 from the supplementary material to the main article (Figure 2).

Table 2 – Typically, I would expect dams to have effects throughout summer and into autumn while they are filling. This extends the summer low flow season further into autumn. Some results here seem to show slightly different autumn results where irrigation return flows are significant.

This was also a result that we expected.

In the reference situation, the discharges in the hydrological network are usually close to 0 at the end of August and for a period of time that depends on the year. Depending on the year, the flows usually increase again in September, October or November (and sometimes later).

In the experiment, we show that, in the autumn, the proportion of network in low flow usually decreases in the impacted situations compared to the reference situation. For us, this means that:

- When flow naturally increase in autumn, the first high-flows are enough to fill all reservoirs quickly, which mean that the disconnections caused by reservoirs disappear when the streamflow increases (note that the usable stored capacity only represents 2.5 % and 5 % of

the mean annual runoff for situations at 140000 m<sup>3</sup> and 280000 m<sup>3</sup> of storage capacity respectively).

- Irrigation return flows can occur even after the end of the irrigation period and increase flows locally and lead to a decrease of the proportion of network in low flow. The conditions and timing in which these return flows occur in the simulations still need further investigation.

An interesting follow up study could be to repeat the study with water withdrawals for stock or domestic use with no irrigation returns, or without the 10% environmental flow requirement which is not required in many other jurisdictions. The results may appear quite different.

As explained above, we cannot turn on or turn off the irrigation return flows. Their importance may vary in other contexts (e.g. deeper soils, different hydrological functioning, more conservative irrigation).

Please note that this article is part of the PhD work of Henri Lechevallier. The question of reservoir management and especially the connection to the stream appears central to us also. Therefore, it will be the focus of a second numerical experiment, which we intend to publish in the future.

I recommend the paper briefly recognises that these results are dependant on some assumptions which may not always be applicable in other locations.

Thank you for this comment, which goes in the same direction than the commentaries of the editor. The revised version of the manuscript has been substantially changed. It now includes a discussion on the generalization of our results to other contexts (sections 4.1.1 and 4.1.2).

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