Remarks from the preceding review file validation:

The ROR database lists the institution of the corresponding author different from the manuscript` s affiliation. Please clarify whether the ROR in the system "Institut National de Recherche pour l'Agriculture, l'Alimentation et l'Environnement (Paris, France)" is still correct.

The corresponding author (Guillaume Thirel) confirms that the Institut National de Recherche pour l'Agriculture, l'Alimentation et l'Environnement (INRAE) is his correct institution. In the ROR database, only the Paris (France) headquarter is available. However, INRAE is a research institute whose researchers work in many diverse labs. In the corresponding author's case, his affiliations correspond to his former lab (Université Paris-Saclay, INRAE, HYCAR Research Unit, Antony, France) and to his current lab (Univ Toulouse, CNES, CNRS, INRAE, IRD, CESBIO, Toulouse, France). Both labs are Joint Research Units with INRAE, and INRAE is Guillaume Thirel's employer.

Editor:

We would like to express our gratitude to Prof Elena Toth for her assessment of the manuscript and the handling of the review process. We provide below detailed answers (in black) to the remarks made by the editor (in blue).

The Authors have submitted a well-organised revised manuscript, providing a detailed response letter that is based on their replies in the discussion, but that also clearly explains how they addressed all the comments in the revision, attempting to add the clarifications we asked for, but without making it too long (in some cases it was indeed a difficult balance…).

The three Referees (that I warmly thank again for their renewed help) and I have all acknowledged the improvement in the presentation allowed by the revision work.

Dr Patan Sopil is satisfied with the current version; Referee #3 asks only to clarify a paragraph in the new text on the classification of the methods that was added in the Introduction ll 83-93 in the not tracked version (104-115 in the track-changes one). And on that section, I agree with Dr. Eekhout that the part on alternative methods is a bit too long and not very clear, whereas the Authors may devote more space to the used storyline approach.

Dr Joris Eekhout has provided a more articulated list of suggestions, in order to clarify some of the newly added text. The main revisions are suggested for better describing the storyline approach: such comments address three different sections: the Introduction, section 2.2.2 -describing the climate projections; and also section 4.3, in reference to the uncertainty issue. The other new text requiring a more substantial revision, with some concepts that need to be clarified, is the last part added in describing the 'complexity of a real-life catchment-scale water resources management' (end of sect. 4.2).

I am sure that the last suggestions by the Referees will further improve the clarity of this paper, so that it may be more fully appreciated by our readership.

Best wishes,

Elena Toth

We thank the editor Prof. Elena Toth for her feedback on the first round of review and on the new comments from the reviewers. We tried to take into account all reviewer's remarks and we believe that these modifications help to improve the clarity of the manuscript.

Reviewer 1:

The manuscript has been revised. Overall, the manuscript has improved, but still major revisions are required. The authors should critically revise the new added text, which is often not easy to understand. This is in particular the case for the new text in the Discussion. See also the specific comments below. Besides, there are two main points that need revisions, which are both related to the storyline approach.

We thank the reviewer for his new assessment of the manuscript and we apologize for the difficulty to understand the added text.

A large part of the Introduction is about the different approaches that are being used in climate change impact assessments. The authors refer to top-down and bottom-up approaches, which have limitations according to the authors. However, a clear description of the bottom-up approach and its limitations is lacking. Then a number of other approaches are mentioned. In the last paragraph of the Introduction the authors mention their approach, i.e. the storyline-based approach, which was not mentioned before. Please describe in 1 sentence what this approach involves and why it is supposed to be a better alternative to the other approaches mentioned in the Introduction.

Thank you for this comment.

The main limitation of bottom-up approaches is that they often rely on simplified stress tests (e.g. deltas of temperature and/or precipitation) that may not be physically consistent with climate model projections, which can weaken credibility. We will add this information.

We added a sentence describing the storyline approach, which is an improvement of the top-down approach as it relies on a smaller set of well-chosen projections.

Regarding what is finally meant with the storyline approach. It turns out that the authors chose 5 climate change scenarios, with differences in annual and seasonal projections in temperature and precipitation. It is, however, not clearly described why these 5 projections are selected. The authors named their approach the storyline approach. So what are the storylines of each of the projections? This should be described more clearly.

Thank you for this comment.

First, we must stress out that we did not invent this approach, as we mentioned in the manuscript by referring to the Shepherd et al. (2018) paper. We just applied it, based on additional recommendations from French climatologists.

We tried to improve the description of the five projections in a story-way.

Below I have provided specific comments to the text, figures and tables.

Thank you for you thorough reading.

Specific comments

Lines 14: Change “develop” to “developed”.

Done.

Lines 21-22: “Only…catchment”, please revise this sentence. It is grammatically not correct and also it is unclear what the authors try to say.

Thanks, we propose to delete this sentence, and to modify the previous sentence as follows: “Moreover, we found that low flows and water demand satisfaction will greatly decline in the future, for four out of the five climate projections we used.”

Lines 22-23: To which negative impacts are the authors referring to? Climate change? And what is not fully satisfactory? The authors mean to say that the adaptation measures are not able to fully reduce the negative impacts? Please revise.

Thanks, we propose the following: “Adapting water uses could help mitigate this decline, though not totally.”

Lines 47-48: From this sentence it seems that all hydrological models neglect water uses, which is obviously not the case. Please revise.

We are rather puzzled by this comment. By definition, hydrological models represent only the natural water fluxes leading to the production of streamflow and other fluxes such as infiltration at a basin scale. Water uses are not processes represented in hydrological models. For example, the Hrachowitz and Clark (2017) paper we cite in this specific sentence, which is a reference paper for hydrological modelling, does not mention water uses at all. When water uses are also considered, we rather refer to integrated water resources management models.

As the reading of the mentioned sentence could be improved, we propose the following: “Hydrological models, as reviewed by Hrachowitz and Clark (2017), classically simulate water resources without considering water uses.”

Lines 50-51: It would be useful to include here a sentence which describes the main difference between conventional hydrological models and IWRM models.

We thank the reviewer for this remark. Such models include water uses and management to the natural water cycle represented by classical hydrological models. We added this sentence.

Lines 54-66: It would be useful to highlight here that WEAP is a lumped model and SWAT a semi-distributed model. Moreover, when reading this paragraph, I'm expecting that the authors use any of the two models, but that is not the case. So why is the model used by the authors not described here?

We found that WEAP is regularly used as a semi-distributed model, see e.g.:

Slaughter, A. R. and Mantel, S. K.: Water quality modelling of an impacted semi-arid catchment using flow data from the WEAP model, Proc. IAHS, 377, 25–33, https://doi.org/10.5194/piahs-377-25-2018, 2018.

Gedefaw, M., & Denghua, Y. (2023). Simulation of stream flows and climate trend detections using WEAP model in awash river basin. Cogent Engineering, 10(1). https://doi.org/10.1080/23311916.2023.2211365

Arranz, R. and McCartney, M. (2007) Application of the Water Evaluation And Planning (WEAP) Model to Assess Future Water Demands and Resources in the Olifants Catchment, South Africa https://www.weap21.org/downloads/iwmi_olifants.pdf

We initially had made the choice not to present airGRiwrm here, as this is a literature review. However, to avoid misunderstanding, we added a couple of sentences at the end of this paragraph mentioning airGRiwrm:

"Another tool, the open source airGRiwrm R package, recently emerged in the IWRM models community (Dorchies et al., 2023). This package relies on the application in a semi-distributed mode of the rainfall-runoff GR models and integrates human water uses (e.g. withdrawals, releases, and dams), as well as water management practices. It was used to investigate future water management at river basin scales (Lemaitre-Basset et al., 2024)."

We however do not provide an extensive description, since this is done in the Methods section.

Lines 73-83: Here the authors describe two different approaches, i.e. top-down and bottom-up. It is relatively well described what is the top-down approach and what is its limitation. However, it is unclear what the authors mean with the bottom-up approach. Moreover, it is not described what is the limitation of this approach.

Thanks for this comment. As mentioned earlier, we improved this part, by modifying the description sentence and adding a sentence on limitations, as follows:

"Conversely, bottom-up approaches focus on risk and socio-economic vulnerability. In these simplified stress tests, possible future climate conditions are often represented by given variations of temperature or precipitation applied to climatic observations to evaluate its socio-economic impact. Their main limitation is that they ignore the uncertainty of the applied variations of climatic variables, which may be inconsistent with climate model projections and weaken the credibility of these approaches."

Line 74: Please change "explosion of uncertainty" to something more scientific.

The term explosion was actually the term used in the cited article. However, as suggested, we replace it with "a large increase in uncertainty".

Line 83: I suggest to start a new paragraph from "An alternative…" onward.

Done.

Lines 95-96: What do the authors mean with "cross-referencing water resources and water uses"? And what is meant with "a posteriori comparison"?

We propose the following: "calculating water resources and water uses separately and then comparing them ». This means that no day-to-day management is implemented in a model, only annual volumes are compared.

Lines 99-100: If this approach (the authors mean incorporation of water uses in hydrological modelling?) is not substantiated by scientific publications, then I suggest to remove this from the manuscript. The manuscript is intended to be published in a scientific journal in which the approaches used should be backed by the scientific literature.

Thank you for this comment. We actually meant the direct incorporation of water uses in hydrological modelling for formulating local water management strategies in France and modified the sentence to make it clearer:

"In a few recent studies, such as HMUC studies (HMUC, for "Hydrologie, Milieux, Usages et Climat" - Hydrology, Environment, Uses and Climate), water uses were directly incorporated into the hydrological models (Etablissement Public Loire, 2024). However, these studies do not explicit their assumptions nor refer to scientific publications, which shows a need for a methodological framework."

We rather disagree with the fact that an approach used in a scientific journal article should be backed by scientific literature, as it would prevent from introducing any new approach. In the present case, as mentioned in the introduction, IWRM approaches rely on some literature.

Lines 103-106: Please revise this sentence. There is nothing novel in applying a model that has been applied for over 10 years. Besides, what is actually meant with "highly performing"?

We realised that a part of the sentence was missing to make it meaningful. We reformulated:

"The novelty of this work first lies in the use in an IWRM approach of a hydrological model, a GR model, which has exhibited high performance for simulating natural flows in numerous water resources, low-flow (Nicolle et al., 2014), high-flow (Berthet et al., 2020), climate change (Thirel et al., 2025) and forecasting (Royer-Gaspard et al., 2024) studies. It also lies in the use of the "storyline-based approach" of Shepherd et al. (2018), since instead of a large ensemble of projections and a statistical analysis as in classical top-down approaches, we used a limited set of climate projections."

What is novel is the use of a hydrological model that is highly-performing for streamflow modelling, for simulating the natural water fluxes in the IWRM approach. Other IWRM approaches generally are not built on such a hydrological model.

By highly performing we mean whose simulated streamflow are known to be very accurate. The GR models have been used in many intercomparison works and are known for their high performance from the hydrological modelling community (see e.g. de Boer et al., 2017).

de Boer-Euser, T., Bouaziz, L., De Niel, J., Brauer, C., Dewals, B., Drogue, G., Fenicia, F., Grelier, B., Nossent, J., Pereira, F., Savenije, H., Thirel, G., and Willems, P.: Looking beyond general metrics for model comparison – lessons from an international model intercomparison study, Hydrol. Earth Syst. Sci., 21, 423–440, https://doi.org/10.5194/hess-21-423-2017, 2017.

106-108: Of course, the details will follow in the Material and methods, but at least the novelty of the used approach should become clear in this sentence. The authors refer to “a limited number of consistent climate futures”, it is not clear enough what is meant by this. Also, how does this relate to the description of the alternatives for top-down and bottom-up approaches in lines 83-93?

Thank you for this comment. We improved this sentence:

Lines 200-201: What is actually meant by “physically-consistent pathways” in the context of the storyline approach? Most readers will assume that that the storylines have a more socioeconomic origin, this is not the case? If this is not the case, then I suggest to change the name of the approach (i.e. storyline) to something more appropriate.

We use here the actual terminology by Shepherd et al. (2018) as referred to in the manuscript.

Lines 203-207: It can be the case that the work for this manuscript has been done in the context of stakeholder consultation or participation, however, by submitting the manuscript to a scientific journal a different approach of presenting the results should be adopted. Looking at the results, there does not seem to be so much difference between the different projections. Moreover, the authors do not present any results related to hydrological extremes. Moreover, there are ways to show the variability within an ensemble.

We kindly disagree with the reviewer here and we are sorry we could not convince him about the interest of this approach.

While it is true that the selection has been done with stakeholder consultation, this does not justify not to present the storylines in a scientific paper. It is also obvious from some figures (e.g. Figure 6 onwards) that the selected projections do present large differences. Low flows, especially when expressed as QMNA5 (the 5-year return period monthly minimal flow) are extremes and are critical for water management. And finally, we never said that there are no other ways to show uncertainty.

We only believe that such an approach, which is more and more used in the scientific community, is developed for real-life applied science, and therefore it is part of the scientific research on which researchers could focus. We however modified the order of the arguments and now begin with scientific arguments before the benefits for the stakeholders:

“Results from the different projections will not be aggregated, since the ensemble mean is a scenario that smoothens extremes, with a much smaller variability than individual models or observations (Gleckler et al., 2008), and so does not represent a potentially real climate (Abramovitz et al., 2014). A few contrasted scenarios are also much easier for catchment stakeholders to apprehend and to use in future planning."

Line 263: Please replace this sentence with: "Gather and analyse measured water withdrawal and release data"

Thanks.

Lines 264-265: Please replace "we do…Santos et al. (2023)" with "please see Santos et al. (2023) for more details".

Thanks.

Lines 266-267: Start this sentence with "Propose…models" followed by ", based on…"

Thanks.

Lines 269-270: "measured water withdrawal and release data"

Thanks.

Lines 271-272: Please explain briefly why this is the case.

Thanks, we added a brief explanation.

Figure 2: Please replace "dam reservoir" with "reservoir". Moreover, please describe in the caption what is shown in the figure, all other details regarding the methods should be described in the text.

As the term "reservoir" was confusing in the first round of reviews, we prefer to keep the term "Dam reservoir" here to differentiate from "small reservoirs". This term is classically used in the literature, see e.g.:

Hoseingholi, P., Moeini, R. & Akbary, M. Optimal Hedging Rules Determination for Dam Reservoir Operation Under Climate Change. *Water Resour Manage* **40**, 107 (2026). https://doi.org/10.1007/s11269-025-04479-x

Hou, J., van Dijk, A. I. J. M., Beck, H. E., Renzullo, L. J., and Wada, Y.: Remotely sensed reservoir water storage dynamics (1984–2015) and the influence of climate variability and management at a global scale, Hydrol. Earth Syst. Sci., 26, 3785–3803, https://doi.org/10.5194/hess-26-3785-2022, 2022.

We reduced the amount of information in the caption of Figure 2 as suggested by the reviewer.

Lines 522-524: Please revise this sentence, I do not understand what the authors try to say here, especially from "which a…not counter."

We rephrased as follows: “The increase in drinking water demand is driven by population growth, and is compensated neither by improved network efficiency nor by the decline in water demand for livestock. “

548: I suggest to change the title to: “On the complexity of catchment-scale water resources management”

Done.

Line 549: Also in this sentence I would remove “real-life”.

Done.

Lines 567-569: It is a bit unclear what the authors try to say here. What do you mean that you did not have access to the type of irrigation in the catchment? You mean that you could not assess which type of irrigation system is being used in the catchment? And you mean that the processes that are accounted for by SWAT+ could have been implemented in the IWRM model? Please revise.

Thanks for this comment. The answer is yes in both cases. We revised the sentences accordingly:

“In our case for example, we never obtained the information about the irrigation system used in the catchment, and although water withdrawal suppressions are public, we had no information about the farmers’ rate of compliance. Similarly to what can be done with SWAT+ (Bieger et al., 2017), providing we had the information about the type of irrigation system, we could have included it in our IWRM modelling.”

Line 570: “It is a challenge to implement the complexity of natural and influenced water transfers into the model.”

Done.

Lines 570-573: There is no need to repeat all the capabilities of the model here. I suggest to remove this sentence and to start the next sentence with “The airGRiwrm tool gives a very complete representation of…”.

Done.

Lines 576-578: With “underground water withdrawals” the authors mean “groundwater withdrawals”? What is actually the relationship between groundwater withdrawals and the exponential reservoir of the GR6J model? You mean that aquifers are not an important source of water within the catchment?

Thanks for these comments. The reviewer is right, we modified the text accordingly:

“The main limitation of our approach is that groundwater water withdrawals were not considered explicitly. They are negligible in the Sèvre Nantaise catchment, due to its crystalline underground. Ongoing work tries to link the exponential reservoir of the GR6J model to groundwater levels (Pelletier et al., 2022), which would allow to incorporate groundwater withdrawals in airGRiwrm, but it is not yet operational.“

Line 581: Please change the wording of “very performing”.

Done.

Lines 584-585: That is why you calibrated the model, why is it important to clarify that here?

Because being calibrated does not ensure that hydrological models are bias free.

Lines 585-586: So what is the benefit of using a conceptual model over a physically-based model? Please elaborate more on this.

We believe that discussing the pros and cons of conceptual models versus physically-based model is not the focus of this work, so we prefer to delete the sentence. Sorry for the confusion.

Lines 613-621: I’m not sure what the authors try to say here. The French government adopted an approach that has been used for a decade now in climate research, i.e. the use of relative temperature increase scenarios, instead of, for instance, RCP scenarios. This approach has not been used in the current study, because the French report on this was published after the current study was performed, but it could have been used. What can the reader learn from this with respect to the uncertainties of the study? Please revise.

The reviewer is right, we used a too long text for expressing a simple concept. We revised it.

“With the exception of the scenario-neutral approach, for which we could prescribe a given temperature increase, decision scaling and info-gap theory are not compatible with the now wide-spread global warming level approach (see e.g. Corre et al., 2025, for France). One critique of the climate projections-based approaches is the large uncertainties. In the current study, there remains a degree of uncertainty, but the objective is to delineate plausible future scenarios rather than calculating probabilities of events or risks of failure. This approach is useful in a catchment with many inter-related water uses, and it is important to keep a consistence between the impacts across the different water use sectors.“

Lines 624-625: Maybe I’m mistaken, but the authors did not choose the 5 used climate projections based on the uncertainty of each individual model. This uncertainty has also not been calculated for all climate projections. So how can this claim be made?

We deleted this sentence to avoid confusion.

Lines 632-633: The authors mean “the GR6J rainfall-runoff model”? What do the authors mean with that the performance of the model was shown in the past?

Yes, we forgot the word “model”, sorry for that.

We mean in past studies. We revised the text.

Line 635: What do the authors mean with efficiency of the modelling framework? This can mean that the model uses little resources (i.e. a fast model) or can have other meanings. Moreover, to which points are the authors referring?

We mean the efficiency in terms of computational time. The points refer to water uses.

We revised the text for both cases.

Lines 640-643: This sentence says that “The impact of water demand evolution…was deemed to be limited on stream flow and water demand satisfaction evolution…”. I do not understand what the authors try to say. Please revise.

We propose the following: “The impact of water demand evolution on streamflow and water demand satisfaction proved limited, compared to the effects of climate change, even under an alternative scenario reducing demand.”

Lines 644-646: Why do the authors make here a comparison with continental and global-scale models? This is the first time that this subject is mentioned. I suggest to remove this sentence from the conclusions.

We added this information to answer a reviewer’s concern in the first round of reviews, but as requested here, we removed this sentence. We added a perspective on the transferability of the approach to other catchments:

“The methodological framework of this study provided water sector managers with tailor-made results to support the design of effective adaptation measures. It could be transferred to other catchments. »

Lines 646-653: Do the results of this study help in this initiative? Or how can this initiative benefit from the results of this work? I’m not sure if this comparison is suited to be included in the conclusions. I suggest to move this to the Discussion section.

As suggested by the reviewer, we moved these sentences in section 4.2 On the complexity of a real-life catchment-scale water resources management.

Reviewer 2:

In my opinion, the authors have done an outstanding job with the manuscript revision. Given the complexity of the topic, the paper's scientific narrative, which was somewhat weak in the original submission, has been significantly improved. The explanations of the IWRM component are much better and will be quite useful for the wider hydrological community.

We thank the reviewer for their inputs from the previous round of reviews and for his/her very positive assessment of the revised version of the manuscript.

One component that might be useful to add toward the end is information on code and data availability. This could be a list of web links to the relevant model codes and packages, as well as to any openly available hydrological datasets used in this paper.

We thank the reviewer for this suggestion. While the complete code used for this study could not be made available as it relies on partly non-public data sources, we agree that providing links towards the hydrological and meteorological datasets as well as the packages might be relevant. We added this information in the new version of the revised manuscript.

Reviewer 3

Dear authors,

Thank you very much for your replies to my previous comments, I consider that the new version of the manuscript addresses them effectively.

We thank the reviewer for their inputs from the previous round of reviews and for his/her very positive assessment of the revised version of the manuscript.

I would just like to suggest a minor revision of the following paragraph:

- Lines 104-115: “An alternative to top-down and bottom-up approaches is the scenario-neutral approach (…)”. I suggest revising this paragraph. For example, scenario-neutral approaches can be framed as part of bottom-up approaches, given that they focus on the vulnerability of the system in relation to a range of potential conditions, regardless of specific scenarios and their inherent uncertainties. See for example van der Laan et al. (2023); O’Shea et al. (2024).
o van der Laan, L., Cholibois, K., El Menuawy, A., & Förster, K. (2023). A scenario–neutral approach to climate change in glacier mass balance modeling. Annals of Glaciology, 64(92), 411-424.
o O’Shea, D., Nathan, R., Wasko, C., Ho, M., & Sharma, A. (2024). Evaluation of key flood risk drivers under climate change using a bottom-up approach. Journal of Hydrology, 640, 131694.

We thank the reviewer for this remark and tried to improve this paragraph.