

Reply to anonymous referee 3

We would like to thank the anonymous referee 3 for his comments on the manuscript, and we regret that the referee does no longer not find the article suitable for publication. We provide a response to each comment hereafter.

Comment:

1. The authors have completed some revisions to address previous comments. Primarily moving content to supplemental information to reduce the size of the paper. However, I still have significant concerns that keep me from recommending publication. Firstly, there is little to no indication of how the contributions of this work can be applied beyond the study region. The methods used are not new, and the results and implications are limited to the study region. Perhaps this work is better suited to a regionally-focused publication.

- Reply: We are sorry that the referee does not consider the paper suitable for publication in HESS, despite the editor and three other referees finding it interesting. One main contribution of this article is the use of a large number of climate projections to drive the AquiFR platform, particularly the use of Regional Climate Models (RCMs). This allows for the best possible accounting of uncertainty from the climate component of the modelling chain, and from the internal climate variability inherent to the mid-latitudes including France. Using such an extensive set of CPs has rarely been done in the literature, in particular dealing with groundwater. Furthermore, using the SPLI as a groundwater metric under climate change is a novel approach that allows comparison of several groundwater tables. The authors propose to emphasise these aspects more clearly at the end of the introduction and in the discussion section.

Comment:

2. Further, it is still unclear to me how these methods differ from previous work - is this the application of established models with climate forcings? Or, is there novelty in some method development or application?

- Reply: As mentioned in the answer for the referee's first comment, the authors propose to emphasise the novelties relying in the article more clearly at the end of the introduction section and in the discussion section.

Comment:

3. Some of these concerns may be a result of language issues - there remain considerable typos, formatting, and organizational issues that make this work quite difficult to follow. I do believe that there is considerable work included in this manuscript, but feel the authors need to clearly discern between what is relevant to the main points of the research, and the relevance beyond the study regions.

- Reply: in order to follow the referee 3's recommendation, the English from the manuscript has been revised. Furthermore, the structure of the manuscript was clarified after the revisions from the referees 1 and 2. Among the changes, the authors moved all the results from all the CPs in supplementary material to let only the ones from the storylines inside the main text, which helped to focus more on the relevant points of this paper, as pointed by the referee 4. As the referee accepted the paper, the authors propose to not add any change to its structure. The authors regret that the referee 3 do not share the opinion of the other referees on this point.

Reply to anonymous referee 4

We would like to thank the anonymous referee 4 for his comments on the manuscript and we are glad he finds this work presents an interest for the hydrogeological community. We propose an answer for each specific comment hereafter.

General comment:

As the other reviewers also mentioned, the rather large spatial variability of the impact of climate change on your groundwater levels makes the key message of the paper unclear. I understand that due to different groundwater systems incorporated in the study there is little room for going in depth on the site characteristics of the different aquifers. The impact of the climate change scenarios on the groundwater seems to have a clear north to south gradient with increasing groundwater levels in the northern catchment and decreasing groundwater levels in the southern catchments, however large spatial differences remain between the different climate change scenarios. As neither the characteristics of the groundwater systems, nor the spatial distribution of the driving forces (groundwater recharge) are described, it is difficult to gain insight into what could be the main reason for this spatial difference in the impact.

- Reply: The referee is right in saying that the large spatial variability in the impact of climate change on groundwater levels makes interpretation difficult. As mentioned in lines 340–341, the study area falls within a transition zone where future trends are wetter in the North and drier in the South. However, the transition zone varies depending on the projection, which is largely due to the uncertainty caused by internal variability (Evin et al., 2025), and more precisely to the chaotic nature of precipitation (Bouttier and Marchal, 2024). This is caused by multiple factors in mid-latitude regions such as France that affect the accuracy of hydrometeorological forecasts (Nuissier et al., 2011), and, in this case, the homogeneity of climate projections. The authors propose to elaborate further this point in section 4.1, to justify why this large variability is to be expected. They also propose to add information about the water balance over the Aquifer domain to investigate the variability over the climate projections (see the answer to the referee's first major comment):

Furthermore, as explained at line 419, this paper alone is insufficient to confirm the existence of a north/south divide due to the southern part of the Aquifer domain being too short. However, as mentioned in lines 420–424, the findings are consistent with those of the EXPLORE2 project's joint research on climate variables (Marson et al., 2024) and surface hydrology (Jaouen et al., 2025; Sauquet et al., 2025; Trambly et al., 2025).

Major comments:

1. In order to increase the understanding of the groundwater system I would like to advise adding some more spatial information. The same type of figure as figure 3 could be used. The following maps could be considered:

- *Change in groundwater recharge (maybe even precipitation and evapotranspiration)*
 - *Temporal dynamics of the groundwater in the model domain e.g p10 – p90 for the reference period, as this might be relevant to interpret the SPLI?*
 - *Change in average groundwater level between the scenarios*
- Reply: as mentioned in the answer to the referee's general comment, the authors propose to add information about the water balance by including results regarding climate variables (i.e., total rainfall and potential evapotranspiration) used to force SURFEX,

and from the associated prognostic variables simulated by SURFEX (i.e., drainage and runoff) used to force the Aquifer modelling platform. The results have been added in supplementary material in a form of three graphics (one per RCP) over the long-term horizon representing the quantiles Q10, Q50 and Q90, to avoid adding too much information in the paper, a concern that was raised by the first three referees.

2. Another aspect that is unclear for me is how the climate change scenarios are forced to the hydrogeological models. Please include if groundwater recharge from the SURFEX model is used in the hydrogeological models using a one way coupling or if feedback mechanisms are considered. Are also other boundary conditions of the hydrogeological models are changing (e.g. river water levels, direct evapotranspiration from the groundwater system...)?

- Reply: the SURFEX land surface model provides the runoff and drainage to the Aquifer platform in a one way coupling, there is no information transferred from Aquifer to SURFEX. The referee is right to highlight this point as feedback mechanisms could ensure a better representation of real evapotranspiration when groundwater level reaches the root zones. However, the contribution of groundwater to the real evapotranspiration does not exceed 2% in France (Decharme et al., 2019), which confirms that neglecting it is deemed reasonable. This point has been clarified in section 2.2 when describing the Aquifer modelling platform.

Furthermore, the EauDyssée and MARTHE modelling packages also simulate river discharges but their simulations were not analysed as the EXPLORE2 project's joint research already deals with surface hydrology (see the answer to the referee's general comment). This point is already mentioned in section 2.2.2. To avoid adding supplementary information to this paper, already deemed long enough, the authors propose to let this section as it is, which mentions that the reader can refer to Vergnes et al. (2020) for more details about the platform.

Minor comments:

1. The background image in e.g. figure 3 has similar colors as the color used for the SPLI change. Can this be improved? Maybe even a polygon around the model area could help.

- Reply: as requested, the background of fig.3 and the associated figures in supplementary material has been modified, by increasing its transparency and adding the contour of the Aquifer domain.

2. Try to limit the use of terminology specific for the region (eg. EXPLORE2 project, EXPLORE 2070 project, SURFEX, MODCOU..), this terminology makes reading difficult for people who do not know these projects or models.

- Reply: the referee is right; the number of terms specific to France is indeed significant. However, these terms are necessary for contextualising the study, particularly with regard to the EXPLORE2 project of which the study forms part. Furthermore, SURFEX, MODCOU, EauDyssée and MARTHE are numerical models that cannot be referred to in any other way. While the authors propose reducing these references as much as possible, it is not possible to eliminate them entirely.

3. If I am correct, you only discuss T10 of figure 4. You could move T5 and T2.5 to the appendix. It might be interesting to include the T10 of figure C1 in figure 4 as you also discuss this figure in the text.

- Reply: the events corresponding 10-years events, i.e., extreme events, are specifically discussed in section 4.3. However, the results from the 5-years events and the 2.5-years events are discussed in section 4.2, along with the other analyses. Figure 4 focuses on the storylines, while Figure C1 in the supplementary material presents the associated analysis from all the available climate projections. In line with the authors' declaration at lines 285–286, they prefer not to mix these results. The solution proposed to fix the issues raised by referees 1 and 2 about the paper being too large was to separate the illustrations, describing the results from the storylines in the main text and providing the results from all the climate projections in the supplementary material. Consequently, the authors propose to let the graphics unchanged as the current version was accepted by the referee 1.

4. Line 361: remove one 'evaluation'.

- Reply: as requested, one word “evolution” has been removed from the main text.

5. On page 17 you mention that Habets et al. (2013) obtained similar results using different methods with the same climate change scenarios. Other studies, however, have obtained significantly different results with different models. I would just mention that you did not investigate the impact of uncertainty resulting from the hydrogeological model.

- Reply: at lines 360–361, the authors state that a proper evaluation of the hydrogeological component of the modelling chain requires a multi-model approach, which is not the case for Aquifer as mentioned at lines 59 in Section 1 and 360 in Section 4.1. This justifies investigating the CDFs from the SPLI calculation to highlight the potential impact of using climate projections in hydrogeological modelling. This is a way of partially investigating the contribution of hydrogeological modelling to uncertainty, albeit in a limited way. This point is described from lines 361 to 378, and the authors do not consider it necessary to describe it further.

6. The part on the different CDF's for the different CP's (paragraph from line 379) is difficult for me to understand. Please include why this is relevant for the comparison of the SPLI's.

- Reply: the referee is right; this point is difficult to grasp. Correctly extracting the CDF over the historical reference period is a critical step in constructing the SPLI (see Section 2.3.1 of the main manuscript). The CDF is indeed used to fit an empirical distribution function (based on a Kernel Density Estimation KDE), on which a standard normal distribution is projected in order to get the SPLI. Therefore, if the reference period is too short or the climate projections introduce too much bias into groundwater levels in the reference period, the CDF and the associated KDE, and consequently the SPLI values, will be affected. The analysis performed in Section 4.1 assesses whether climate projections induce deviations in the CDFs that are large enough to generate biased SPLI values and thereby bias the interpretation of future groundwater levels. The authors propose to clarify this point at the lines 370-378 in section 4.1.

7. Some of the figures that were not included in the paper, but that are mentioned in the text (e.g. figure 6 for different months) could be included as supplementary information.

- Reply: the figures mentioned in lines 382–383 are not included in the main text or supplementary material, as the results do not differ much from those illustrated in Figure 6. To avoid adding too many results, a concern raised by the first three referees, the authors have decided not to include them as they do not provide much new information.

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