

## **Reply to anonymous referee 2**

We would like to thank the anonymous referee 2 for his truly relevant comments on the manuscript and we are glad he finds this work may show an interest for the hydrogeological community, and may be worth publishing. We agree with most of the comments and we propose an answer for each specific comment hereafter.

### **Major Comments**

#### **Comment:**

*1. The paper focuses on the impact of climate change but do not address – except at the very end of the conclusion – the strong link between water availability and water demand. One could expect agricultural and water drinking demands to change with climate change, potentially strongly impacting groundwater level. I feel like this issue should be mentioned earlier in the paper and discussed a bit more.*

- Reply: the referee is right, one can suppose that water demand is expected to change accordingly to the impacts of climate change on water supply. However, addressing anthropisation impacts of water resource was not one of the objectives of the EXPLORE2 project, of which this work is part. Indeed, the idea was to identify climate change impact and anthropisation impact separately, in order to help users to engage into adaptation measures. This point was mentioned in the manuscript as a major limitation of the study (lines 436 to 441). However, the question was assessed on a recent study by Guillaumot et al. (2024). As requested, the authors propose to clarify in the introduction part that this point will not be addressed in this study.

#### **Comment:**

*2. The presentation of the results is hard to follow because of the 36 scenarios used and the 4 storylines. I wonder if treating the 4 storylines – that are specifically chosen to illustrate the potential contrasting futures – alone would not be sufficient to give a clear and synthetic message. Especially because the simulated groundwater levels are very uncertain with the 36 scenarios.*

- Reply: the referee is right. The authors propose to analyse the results from the storylines in the main text and to provide the analyses on all the CPs in supplementary material. However, the authors think that fully removing the analyses on all the CPs is relevant as both sets of analyses are complementary and should not be dealt with separately. This point of view is shared by the EXPLORE2 project community, to which this work is part. A sentence explaining this choice will be provide in the main text after the exploitation of the results on SPLI median from all the CPs.

#### **Comment:**

*3. I feel like treating the RCP 2.6 in the paper is not relevant. Because the current CO2 emissions trajectories make it very unlikely, and because the results for this specific RCP are so uncertain that no conclusion can be drawn. I would remove it for sake of simplicity.*

- Reply: the referee is right on the fact that the current CO<sub>2</sub> emissions trajectories make RCP2.6 very unlikely and that the results induced by RCP2.6 are too divergent. However, the authors propose to move them to supplementary material, to simplify the main text but to still keep them closed to the study, to highlight that the divergences observed from the results mostly depend on the GCM/RCM couples than on the RCPs, which is a difference between climate variables and hydrological variables (Evin et al., 2024).

Comment:

*4. The way SPLI is computed is not clear enough for me. The way the normal CDFs are transposed in SPLI distribution should be explained.*

- Reply: the referee is right, the description of the SPLI was not clear enough. The calculation of the Standardized Piezometric Level Index is similar to the calculation of the Standardized Precipitation Index (SPI) (McKee et al., 1993). The SPLI is an indicator used in the French Monthly Hydrological Survey published each month. Details about its computation are given in Seguin (2015). Considering a piezometric head time series of N years, the steps are the following:
  - Step 1: the monthly mean time series from the reference data is computed;
  - Step 2: constitution of twelve monthly time series (January to December) over the N year period. For each time series of N values, a non-parametric kernel density estimation (KDE) allows estimating the best probability density function (pdf) fitting the reference histograms. As time series of piezometric heads show a big variety of histogram, the use of a KDE to estimate a pdf fitting the reference histogram is preferred;
  - Step 3: For each month from January to December, the adjusted cumulative density function is deduced, and a projection over the standardized normal distribution allows deducing the SPLI.

This part will be clarified in the main text, section 2.3.1, and Figure 2 will be redrawn to better illustrate the calculation method.

Comment:

*5. On all the maps, I would use a very distinct color for the part of France that were not simulated. Figure 3 shows a lot of untreated scenarios. Consequently, the maps for the treated one are very small and hard to read. I don't know how to improve that but having larger map would be great.*

- Reply: the referee is right, Figure 3 was unclear. The authors propose a new version (see Figure 1 below) of this type of figures (Figure 3 and the associated elements in supplementary material) to increase the size of each element while keeping the matrix showing the different impacts from either the GCMs or the RCMs. Moreover, the couples used as storylines will be highlighted in this new version, in order to delete the figure dedicated only to the storylines and to simplify the main text.

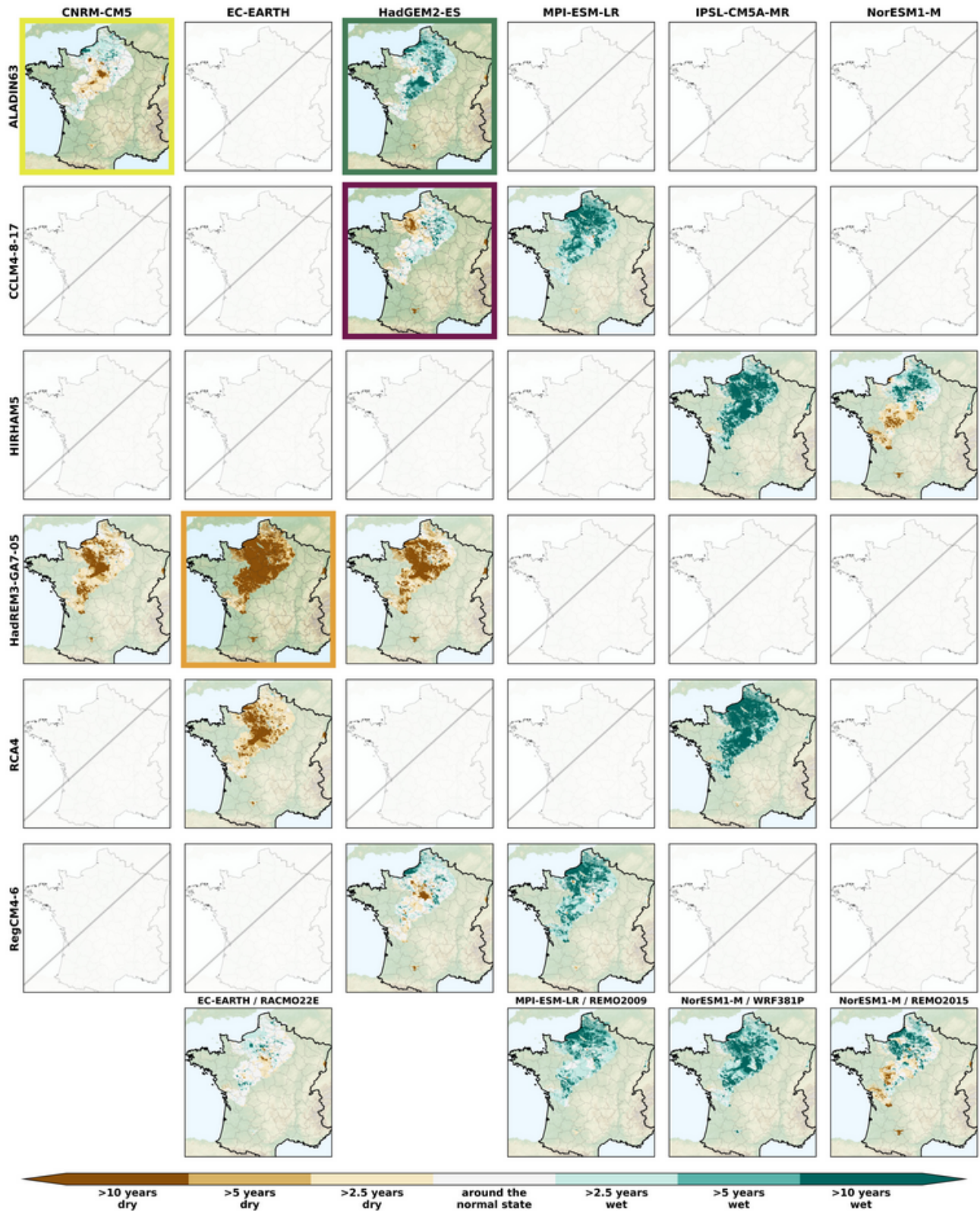


Figure 1: Spatial distribution of the SPLI median over the AquifR domain under the **RCP8.5** on the long-term horizon (2071-2099), following the different GCM (in column) / RCM (in line) combination. The last line corresponds to couples where RCMs are used only once. The colored rectangles indicates the GCM/RCM couples corresponding to the storylines.

Comment:

6. *Figure 5 is really dense, and should be looked at together with Table 5 to follow the text. It's thus quite challenging to catch all the information's presented in part 3.2. Maybe one more argument to keep only the storylines.*

- Reply: as requested and as mentioned above, Figure 5 will be replaced by the same version focused on the storylines, and the analyses from all the CPs will be provide in supplementary material. Table 5 will also be put into supplementary material to avoid redundant elements and clarify the main text.

Comment:

7. *I don't really get to what extend the results presented in Vergnes et al, 2023 are different from the MARTHE part of the submitted paper. I would appreciate if this could be discussed somewhere in the introduction.*

- Reply: the MARTHE software package used in Vergnes et al. (2023) is based on a simple configuration, with only one layer describing groundwater and a resolution of 2km x 2km, which is larger than the MARTHE configuration used in this paper. Furthermore, the analyses provided by Vergnes et al. (2023) are based on only 5 CPs while this study used 17 GCM/RCM couples, which bring more information specifically regarding uncertainties from the CPs. This point constitutes the main difference between the two studies, and was mentioned in the paper (lines 422-427). The authors propose to clarify the corresponding section to make this point clearer, and also to mention it earlier in the introduction.

Comment:

8. *In part 2.2.1, it's not very clear whether SURFEX or ISBA or both are used for the study. This part is rather confusing and unclear. I think it should be improved.*

- Reply: the referee is right, this part in unclear. ISBA is the land surface scheme dedicated to the interaction between the atmosphere, the soil and the biosphere. It is part of the SURFEX modeling platform which is basically designed to integrate several surface schemes such as ISBA, TEB for cities, SEA for sea (Le Moigne et al., 2020). The authors propose to clarify this point in the main text.

Comment:

9. *Multi-model is advocated in the introduction. One could expect that this approach is used throughout the study. It is clearly done for the climate part but not for the groundwater modelling part. If I get it right, most of the time MARTHE and EauDyssé were not applied on the same region. And it's hard to get where it's been done, as all the surfaces in Table 3 are different. I think the reasons why only one groundwater model has been applied should be stated somewhere. And Table 3 should be improved so that locations where both models are applied appear clearly.*

- Reply: the referee is right, this point is unclear. Strictly speaking, we cannot say that multi-model approach is used for hydrogeological modelling. Some applications do share parts of their spatial coverage, e.g., the Somme region, but they are few and their configuration (i.e., their assumptions, spatial resolution and spatial coverage) makes them not directly comparable. Furthermore, multi-model approach has already been analysed in the RexHySS project (Habets et al., 2013), which showed on the Somme and the Seine regions that the elements from the modelling chain bringing most of uncertainty are the climate projections. Then the choice was made not to consider it here, in addition to the technical difficulty of implementing several hydrogeological models for each application of the Aquifer platform. The authors propose to clarify this point in the introduction section, and to add a comment in the discussion (section 4.1) to remind why hydrogeological multi-modelling was not used.

## Minor Comments

### Comment:

1. *First sentence of the introduction is not clear for me. Especially “for both agricultural purposes”. Please rephrase.*

- Reply: as requested, this sentence will be rephrased.

2. *Line 83: typo – replace dot by coma*

- Reply: as requested, this issue will be addressed.

3. *How are the weather regimes defined?*

- Reply: the weather regimes are based on the cluster of the geopotential height at 500 hPa of the ERA-40 analysis (Verfaillie et al., 2017), on a seasonal basis: (1) December, January, February (DJF); (2) March, April, May (MAM); (3) June, July, August (JJA); (4) September, October, November (SON). The weather regimes are based on the 500 hPa geopotential height cluster of the ERA-40 analysis. This point will be clarified in the manuscript.

4. *Line 117 : typo (Vergnes et al, ...)*

- Reply: as requested, this issue will be addressed

5. *Line 128 : typo - to simulate*

- Reply: as requested, this issue will be addressed

6. *Line 148: “vertical exchange through aquitards” – not clear for me. Aquitards are not supposed to conduct water.*

- Reply: the referee is right, hydraulic conductivity in aquitards can be very low. However, aquitards can also be semi-permeable and characterised by low (but not null) hydraulic conductivity. Consequently, vertical exchange through such aquitards are taken into account in SAM in order to integrate confined aquifers.

*7. The way unsaturated zone is treated in MARTHE should be detailed.*

- Reply: the referee is right, the unsaturated zone in MARTHE is not fully described in the applications used, and a lag is simulated in simple places using a reservoir approach. However, the authors think that it is not necessary to go into further detail on this point in order not to make the main text more voluminous, in particular as requested by referee 1. The reader is encouraged to refer himself to Habets et al. (2021) for further details, as mentioned at the line 160.

*8. Line 228: I don't get what is meant by "physical consistency of each storyline is conserved"*

- Reply: the sentence was unclear. The focus on storyline makes it possible not to present means and statistical results, but to present few plausible futures with all their physical consistency (Marson et al., 2024). The approach is therefore more deterministic regarding the scenario represented by the storyline. The point will be clarified in the manuscript.

*9. Line 304: typo "Figure 6 the maps". Please rephrase*

- Reply: as requested, this issue will be addressed.

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