Author response to Referee 1

We thank the referee for the very thorough, constructive, and insightful review. We appreciate the positive assessment of the novelty and practical relevance of the proposed method, as well as the detailed suggestions for improving clarity, methodological transparency, and the discussion of limitations. We fully agree that several aspects of the method require clearer explanation and that the limitations, particularly those related to snow cover, seasonal coherence, and temporal variance, need to be discussed more explicitly. Below, we respond point by point to all comments and outline how they will be addressed in a revised version of the manuscript.

Response to major comments

1. Clarity of the methodological description

Referee comment:
The method is not always clearly explained, and the details are sometimes difficult to follow. The authors should therefore improve the description of the method (see the minor comments for the points where improvements are particularly necessary).

Author response:
The referee correctly notes that parts of the methodological description are difficult to follow in the current manuscript. In the revised version, we will improve the presentation by providing a clearer step-by-step description of the workflow, by stating the key assumptions more explicitly, and by revising figure references and captions so that the figures can be interpreted more easily on their own. Specific points raised in the minor comments will be addressed individually to improve readability and transparency.

2. Use of seasonal years versus hydrological years

Referee comment:
The limitations of the method concerning snow cover, and the estimation of cold, dry, or wet spells in winter, which could be disrupted when years are reassembled, should be better discussed and illustrated. For example, snow cover in January will have nothing to do with the snow cover from the previous December, which could be a major issue for some applications.

As I understand, the authors use standard calendar years (January to December), but it would make more sense to at least start the year in December so that the winter averages in the regional projection retain climatological meaning (i.e., to avoid stitching December to January and February from another winter). At the very least, the snow season would not be artificially split. More generally, I think that for many applications, using "hydrological years" (e.g., years starting in October for example) would help avoid issues related to snow and reduce problems related to soil moisture. What is the rationale for using standard years?

Author response:

The referee raises an important point regarding the treatment of winter seasons. In the current implementation, years are defined from December to November, such that December is grouped with the following winter months. This choice was made to avoid splitting winters across two different reconstructed years and to preserve seasonal coherence as much as possible.

That said, this definition is not optimal for all applications. In the revised manuscript, we will more clearly justify the December–November choice, explicitly discuss its limitations for snow cover and land-surface memory, and outline alternative implementations (e.g. hydrological years) as a natural extension of the method. Such alternatives could further reduce artifacts related to snow and winter processes.

3. Limitations related to snow cover, winter processes, and temporal variance

Referee comment:
"The limitations of the method concerning snow cover, and the estimation of cold, dry, or wet spells in winter, which could be disrupted when years are reassembled, should be better discussed and illustrated. For example, snow cover in January will have nothing to do with the snow cover from the previous December, which could be a major issue for some applications. Also, since some RCM years can be repeated, what is the impact on temporal variance at different time scales: daily, seasonal, and decadal? The authors should analyze at least some of these limitations, and discuss all of them thoroughly."

Author response:

We thank the reviewer for raising these important points. The backbone of our approach is that we exclusively use existing RCM output, intervening only by removing or repeating — but never placing duplicated years back-to-back — individual annual segments to match the target warming trajectory (here based on CMIP6 GCMs). This resampling inevitably introduces discontinuities at the join points between reassembled years. In the revised manuscript, we will include supplementary diagnostics illustrating the magnitude of these discontinuities (e.g., jumps in snow depth or soil moisture at the segment boundaries) and quantify their severity at different timescales.

For most typical analyses at monthly timescales and beyond, these discontinuities can effectively be neglected, because the method preserves the statistical distribution of the underlying climate. Importantly, there are no dominant quasi-periodic modes of variability in the European domain that operate at timescales where the resampling pattern could systematically bias the sample (for instance, by preferentially removing years corresponding to one phase of an oscillation). Interannual variability in the RCM is thus sufficiently uncorrelated that removing or duplicating non-consecutive years does not meaningfully distort the climatological

distribution. We will demonstrate this explicitly in the revised manuscript. We further note that the method is designed to be applied to an ensemble of RCM projections. In this context, unsystematic artifacts from the resampling of any individual simulation are further diluted across the ensemble, i.e. the aforementioned caveats are less consequential for the primary intended use case.

However, for analyses that depend on temporal sequence rather than aggregated statistics, such as consecutive dry days, snow cover persistence, or winter cold spells, the choice of the month at which annual segments are split becomes important. For snow-related indices, e.g., a mid-summer split avoids breaking up the snow season, whereas for precipitation-based indices such as consecutive dry days, splitting during the wet season is recommended. We also note that in individual cases, a particularly extreme year may be duplicated, which could bias analyses specifically targeting extreme event statistics; users should verify that such cases do not affect their results.

In the revised manuscript, we will provide explicit application guidance, distinguishing between analyses for which the resampled data can be used without concern (e.g., long-term climate statistics at monthly timescales and beyond), cases where the choice of segment boundaries matters and appropriate settings are recommended, and analyses where particular caution is warranted (e.g., extreme event attribution from individual simulations).

4. Assumption of accurate European warming in GCMs

Referee comment:

The method also makes the implicit assumption that the regional warming of GCMs over Europe is accurate (for a given level of global warming), but an increasing number of studies suggest this is not the case, i.e. that GCMs themselves tend to underestimate warming over Europe. The method proposed in this paper corrects for the fact that CMIP6 models, as an ensemble, may overestimate global warming, but it does not account for the possibility that they underestimate warming over Europe. As a result, the global "cold" correction may amplify the regional underestimation of warming in the final adjusted projections. This should be noted and discussed.

Author response:

We agree that this point deserves to be explicitly stated in the revised manuscript. In a standard projection setting, even though RCMs develop their own internal variability and simulate regional processes, they are still fundamentally constrained by what the driving GCM prescribes at the boundaries, and thus largely inherit its large-scale warming response. Our method builds on the same assumption; in fact, we consider it an asset to rely on the best available global climate models down to the region scale, and zoom in using regional climate models.

Nonetheless, alternative strategies that attempt to observationally constrain regional warming do exist (e.g. Ribes et al. 2022), Corre et al., 2025), and could in principle be combined with our framework by using constrained regional warming rather than CMIP6 GCM output as the target. While this remains out of scope for our study, we will mention this possibility in the adjusted manuscript.

Ribes, A., Qasmi, S., and Boé, J.: An updated assessment of past and future warming over France based on a regional observational constraint, *Earth System Dynamics*, 13, 1425–1447, https://doi.org/10.5194/esd-13-1425-2022, 2022.

Corre, L., Boé, J., and Somot, S.: Using regional warming levels to describe future climate change for services and adaptation: Application to the French reference trajectory for adaptation, *Climate Services*, 38, 100523, https://doi.org/10.1016/j.cliser.2024.100523, 2025.

Response to minor comments:

l48-52
RC: *This paragraph is not very clear. It could be useful to better explain the method of Tebaldi et al. (2022), as it is used here and at other places as a reference for the explanation of the authors' method. For example, "harmonizing existing high-resolution regional climate projections with updated global and regional warming constraints" is not clear or precise.*

Author response:
This paragraph will be clarified in the revised manuscript. The description of the method of Tebaldi et al. (2022) will be made more explicit, and its relation to the approach used here will be stated more clearly.

Introduction – additional references and climate services context
RC: I think it would be useful to cite Ribes et al. (2022), who show another line of evidence for the underestimation of regional warming over Europe (France in that case) by RCMs (and the GCMs). Also, it would be interesting to discuss how climate services across Europe try to deal with the issue of reconciling global and regional projections (e.g., Corre et al., 2025, for France). I also think that the approach of Corre et al. has the advantage of taking into account the underestimation of regional warming over Western Europe by all models (global and regional), compared to the approach proposed by the authors. This could also be discussed in the conclusion, for example.

Corre et al. (2025) Using regional warming levels to describe future climate change for services and adaptation: Application to the French reference trajectory for adaptation. Climate Services

Ribes et al. (2022) An updated assessment of past and future warming over France based on a regional observational constraint. Earth Syst. Dyn

Author response:
We thank the referee for these suggestions. We will cite Ribes et al. (2022) and Corre et al. (2025) in the revised manuscript and discuss their relevance in the context of underestimation of regional warming over Europe and the approaches used by climate services to reconcile global and regional projections. We will also briefly compare their conceptual advantages and limitations with respect to the approach proposed here, including in the conclusions.

L72
RC: *SSP5-8.5 and RCP8.5 are not exactly identical, which can lead to some differences. The authors should discuss this and cite relevant literature.*

Author response:
This distinction will be clarified in the revised manuscript, together with a brief discussion of its implications and appropriate references.

L82
RC: *The authors should provide the exact properties of the locally weighted regression: which order is used? (e.g., linear or quadratic?).*

Author response:
The regression method will be specified more explicitly. In particular, we will state that a LOWESS regression is used and summarize the relevant parameters.

L87
RC: *Which markers? They are hard to see in the figure. Also, I don't see repeated years at the beginning of the mapping. The RCM years seem to increase from the beginning to 2075, so why are they filtered (in grey) at the beginning? The authors should clarify this point.*

Author response:
Figure 1 will be revised to improve marker visibility. The text and caption will also clarify which points correspond to repeated or filtered years and why filtering occurs at the beginning and end of the mapping.

l89–92 (grouped: linearization and year mapping)

RC: *Not clear. Also, I don't understand exactly how the linearization works or why a long window of 20 years is needed. Also, the evolution of years in Fig 3c3 does not seem totally linear over 20 year periods. The sentence on line 91 that justifies this is not clear. And is there not a risk of inaccuracy by forcing all the 20-year lines to connect. ? Additionally, the regression does not provide entire years I suppose, but years with decimals (e.g., 1998.54). Please explain how this is dealt with.*

Author response:

We thank the referee for the detailed comments. The revised manuscript will clarify that the linearization is applied to map model years to target warming trajectories by approximating the cumulative warming over multi-year segments, which smooths interannual variability while preserving the overall trend. A relatively long window (20 years) is used to reduce noise and ensure stable estimates of the year mapping, though we note that some non-linearity remains within each segment. Non-integer years resulting from the regression are rounded to the nearest complete year when selecting annual fields, and the procedure is designed to ensure approximate continuity between segments. We will add a brief explanation of these points to make the method and its limitations clearer to the reader.

Figure 1c, panels 3 and 4

RC: *It is better to avoid placing the legend over the curves.*

Author response:
The figure layout will be revised accordingly.

L102

RC: *Is it always possible to enforce a 0.75 slope in the GCM-RCM mapping? If the RCM warms much slower than its driving GCM, how is it possible to maintain a 0.75 slope? What happens if it is not the case?*

Author response:
This point will be clarified. The revised manuscript will explain that the minimum slope constraint can always be imposed as a regularization step, but that when the RCM warming rate strongly exceeds that of the driving GCM, the resulting mapping is no longer meaningful. In such cases, the validation step based on the evolution score truncates the affected part of the RCM time series.

L104–107

RC: *Not clear. And what is the scientific justification for this? Why do the authors avoid consecutive duplicate years if it is the best choice to match the GCM?*

Author response:

We agree that the rationale is not sufficiently explained. In the revised manuscript, we will clarify why consecutive duplicate years are avoided, how the iterative swapping procedure works, and discuss its implications for temporal variability and physical consistency.

comments near l125 (grouped: memory effects and winter discontinuities)

RC: *The limitations of the method could be better explained. If I understand correctly, 31/12/2012 could be followed by 01/01/2011 or 01/01/2013. In any case, there is a discontinuity, with possible large and unphysical shifts in snow cover (as we are in winter). This should be more clearly mentioned (rather than referring vaguely to "memory issue"). Would it not have been preferable to start years in fall, for example, to limit this issue with snow cover (and soil moisture) i.e. to use "hydrological years"? Also, if someone is interested in cold, wet, or dry spells in winter, this will be problematic. This should be noted.*

Author response:

The revised manuscript will explicitly state that consecutive years in the adjusted time series may originate from different original winters, leading to discontinuities in snow cover and other memory-dependent variables. Implications for analyses of winter cold, wet, or dry spells will be highlighted, and the potential benefits of alternative year definitions will be discussed.

Section 2.3

RC: *I'm not sure to understand the point of section 2.3, as no result of the "validation" described is shown.*

Author response:

We agree that the purpose of Section 2.3 is currently not sufficiently clear, especially since the cross reference for the visualization in the attachment is missing. In the revised manuscript, we will clarify its role and cross reference the explicit validation results illustrating the evolution score.

l130 (grouped: truncation and ensemble consistency)

RC: *Is it not an issue to have to truncate the adjusted time series? Information from the same ensemble of RCMs cannot be used consistently through time periods because each regional climate simulation has to be truncated at a different time. Is this not an issue, for example, when characterizing model uncertainties, as the ensemble of models considered changes with time?*

Please also provide a table (or a figure) with the last (and first) year of each adjusted RCM simulation.

For example, how can the adjusted time series in Figure 2 be extended to 2100? In Figure 4, when the text refers to 2060, what is the exact time period represented? Is it for example a 20-year average (e.g., 2050–2070). Are all the adjusted regional projections available until 2070, or is there a cutoff earlier for some simulations? Is it not an issue?

Author response:
We acknowledge that truncation of the adjusted time series affects the temporal consistency of the ensemble. In particular, not all RCM simulations extend to the same end year, which can influence ensemble statistics and uncertainty estimates at later times. In the revised manuscript, we will clarify which time periods are represented in each figure (e.g., whether values correspond to 20-year averages) and note that some simulations are truncated before the end of the period shown. We will also provide a summary of the temporal coverage of the adjusted simulations to make clear the periods for which each model contributes to the ensemble, highlighting any implications for interpreting ensemble trends and uncertainties.

L136
RC: *What is the size of the window to compute the slope here? Is it 20 years also? Are they running windows?*

Author response:
Yes, the slopes are computed using the same 20-year running windows. For each position, a linear regression is fitted over a 20-year segment of both the reference and adjusted series, and the resulting slopes are compared. The window is shifted by one year at a time, and each slope is associated with the center year of the corresponding 20-year period. We will clarify this explicitly in the manuscript.

Figure 1 panel numbering
RC: *The numbering is inconsistent, with panels labeled as a, b, and then c1, c2, c3, etc.*

Author response:
Panel labeling will be corrected to ensure consistent and unambiguous numbering.

l150–170 (grouped: global warming adjustment and interpretation)
RC: *It is not really clear how the adjustment for global warming is done. The explanation should be detailed, explained step by step.*

Also, when the adjustment is applied to the 95th percentile from the IPCC estimate, is the ensemble mean of CMIP6 models adjusted to the 95th percentile? If so, the resulting

multi-model distribution extends well above the IPCC 95th percentile, which does not really make sense, I think. Am I misunderstanding something?

Note: Figure 3 is not particularly clear (especially 3a). Also, please add the ensemble spread in Figure 3c with shading.

Author response:
This section will be clarified by providing a more explicit, step-by-step description of the global warming adjustment procedure. The application of the IPCC best estimate and 95th percentile trajectories, as well as the interpretation of ensemble spread after adjustment, will be clarified. Figure 3 will be revised accordingly.

Conclusions
RC: *Please discuss the limitations of the method more thoroughly in the conclusion.*

Author response:
We will expand the conclusions to more explicitly and comprehensively discuss the limitations of the method, including issues related to seasonal coherence, snow cover, temporal variance, truncation, and applicability to different types of climate impact analyses.

typographical errors
RC: *I also found quite a few typos. A careful proofreading prior to the submission of the new version of the manuscript could be beneficial.*
"chatateristics" (l164)
"trajectoeies" l165
"warming-adjustement" (Fig. 3 caption)
"tragectory" (Fig. 3 caption)
"warming responsedatasets"" (Fig. 4 Caption)
"temperatues" (Fig. 4 caption)
"unter" (Fig. A1 caption)
Figure A1, title: 20250 instead of 2050
"componentn" (Fig. A2 caption)

Author response:
We thank the referee for identifying these typographical errors. All listed typos and figure caption errors will be corrected in the revised manuscript.