Response to Reviewer 4 comments about the article "*A Bayesian Statistical Method to Estimate the Climatology of Extreme Temperature under Multiple Scenarios: the ANKIALE Package*"

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> **Note** In this document, the text in regular format corresponds to the reviewers questions. The answers from authors are given in the grey blocks.

1 Reviewer 4 (Anonymous)

1.1 Intro

This paper extends previously published methods to estimate the changing likelihood from the past to the future of a specified extremes metric due to historical forcings and future emission scenarios. In addition the likelihoods from a counterfactual world, one without anthropogenic emissions is estimated. The new extension to the methodology is enabling a single counterfactual world to be estimated from multiple different scenario runs. The authors also revise the Bayesian approach to reflect advances in this field. For the analysis the authors have developed a software package that is publicly available to facilitate other researchers wishing to perform similar tasks.

> We would like to thank Reviewer 4 for this summary and for recognizing our efforts to make a Bayesian-based statistical analysis method for extreme events accessible to as many people as possible.

My request for major revision is not due to significant concerns of the analysis they have performed but because of the lack of placing their work in the context of the field more generally and the belief that in its present form the text is only accessible to a very small audience. I found what the authors have exactly done and why quite difficult to ascertain and feel that I have only got the general gist rather than a good understanding.

> We are sorry that you have not gained a good understanding of the method. We have deeply reworked on the text (particularly Section 3) with this in mind, and including the suggested references in particular.

1.2 Context of other work

There is now quite a body of work addressing the production of posteriors of climate variables from the combination of observations with climate models more generally and extreme specific. It would be helpful if they placed their work in the context of these other pieces of work.

A discussion has been added to provide much more context to the present work.

1.2.1 General Bayesian climate projection

- A. Ribes et al. (Jan. 2021). "Making Climate Projections Conditional on Historical Observations". In: *Sci. Adv.* 7.4, eabc0671. DOI: 10.1126/sciadv.abc0671)
- C. J. Smith et al. (2021). "Energy Budget Constraints on the Time History of Aerosol Forcing and Climate Sensitivity". In: *J. Geophys. Res. Atmos.* 126.13, e2020JD033622. ISSN: 2169-8996. DOI: 10.1029/2020JD033622
- G. C. Hegerl et al. (June 2021). "Toward Consistent Observational Constraints in Climate Predictions and Projections". In: *Front. Clim.* 3. ISSN: 2624-9553. DOI: 10.3389/fclim.2021.678109
- L. Brunner et al. (Oct. 2020). "Comparing Methods to Constrain Future European Climate Projections Using a Consistent Framework". In: *J. Clim.* 33.20, pp. 8671–8692. ISSN: 0894-8755, 1520-0442. DOI: 10.1175/JCLI-D-19-0953.1
- L. Brunner et al. (Nov. 2019). "Quantifying Uncertainty in European Climate Projections Using Combined Performance-Independence Weighting". In: *Environ. Res. Lett.* 14.12, p. 124010. ISSN: 1748-9326. DOI: 10.1088/1748-9326/ab492f
- J. M. Murphy et al. (2018). *UKCP18 Land Projections: Science Report.* Tech. rep. Met Office Hadley Centre
- R. Knutti et al. (2017). "A Climate Model Projection Weighting Scheme Accounting for Performance and Interdependence". In: *Geophys. Res. Lett.* 44.4, pp. 1909–1918. ISSN: 1944-8007. DOI: 10.1002/2016GL072012
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- G. R. Harris et al. (June 2013). "Probabilistic Projections of Transient Climate Change". In: *Clim Dyn* 40.11, pp. 2937–2972. ISSN: 1432-0894. DOI: 10.1007/s00382-012-1647-y

1.2.2 Extreme specific probabilsitic projection

- J. M. Murphy et al. (2020). *UKCP Additional Land Products: Probabilistic Projections of Climate Extremes.* Tech. rep. Met Office Hadley Centre
- S. J. Brown et al. (Nov. 2014). "Climate Projections of Future Extreme Events Accounting for Modelling Uncertainties and Historical Simulation Biases". In: *Clim Dyn* 43.9, pp. 2681–2705. ISSN: 1432-0894. DOI: 10.1007/s00382-014-2080-1

1.3 Accessibility

I do not underestimate how difficult this challenge is and it is difficult to know how to advise on this. Apart from the hurdle of the Bayesian terminology I think the reader will struggle to put all the pieces together. This could be alleviated with some text at the beginning of the Methods section giving an outline of the whole procedure, how the different parts fit together, the assumptions of the approach, how climate model deficiencies are accounted for and how biases with observations are dealt with.

For those readers who will probably never be that comfortable with the more statistical aspects of the paper I think they will be greatly helped if there was more emphasis and care in the physical interpretation of the method and discussion of the example analysis. Also, a comparison with other posterior work who's emphasis is more towards the physical modelling uncertainty of future climate For example such a discussion would include the impact of this being an ensemble of opportunity, how carbon cycle uncertainty and aerosol modelling uncertainty is samples and their consequences for the results presented. Also a discussion on the relative importance of X_R and X_G, what different do they bring to the analysis

Thanks for your interesting suggestions. The entire Section 3 has been entirely rewritten in this regard in order to make it much more accessible.

1.4 Other general comments

1. I know section 5 is a really important part of the paper but it really does interrupt the flow. Might the authors consider moving it to an appendix?

   Part of the message of this paper is precisely to promote this tool (and that is one of the reasons why we chose GMD), so we do not want to move it to the appendix. However, we have reworked it to make it easier to read.

2. Are return periods and their changes particularly helpful due to their extremely nonlinear behaviour? The issue with looking at return periods in the present day is that the return period will be dominated by uncertainty in the shape. The actual increase in extreme temperatures, however, will be dominated by the change in location. Present day factual and counterfactual comparisons if looking at return periods will be dominated by shape uncertainty whereas present to future comparisons of the 100y return level will be dominated by location change and its uncertainty.

   These crucial questions go beyond the scope of this article, which focuses on presenting a tool for estimating return periods based on the GEV model commonly used in this context. They have been be mentioned in the new conclusion, and it would be particularly interesting to compare this with statistical models that strongly constrain the shape through the upper bound (Noyelle et al., 2025).

3. The adaptation sphere is very focussed on high resolution modelling of the future climate (perhaps too much). As this paper is also concerned with determining the likelihoods of future extremes it would be useful if the authors could comment on how their method might accommodate regional climate modelling.

   This has been discussed in the new conclusion, with the idea that regional models can be integrated in the same way as GCMs.

4. X^R and X^G - these will be highly correlated. Can you demonstrate that both are required? If you only had one will not the parameters not just get adjusted to compensate? Or put it another way, does the small bit of extra info when using both lead to a significantly better outcome?

Indeed, especially for the attribution of current events, using X^G or X^R as a covariate will not change the result much (which is what the WWA typically does). Now, looking ahead to 2100, even though both series start at ~ 0 in 1850 (in terms of anomaly), the date on which the trend emerges is not the same (due to aerosols) and the end point is not the same (it is warmer in Europe than globally). We believe that these differences would be significant for local temperatures, which depend more on regional than global temperatures. The idea here of keeping both covariates is to be able to address the dependence between global, regional and local warming. This makes it possible, for example, to examine regional and local warming if global warming is fixed at a certain value.

1.5 Minor comments

1.5.1 Abstract and Intro

Abstract general

- could be improved to better describe the solution it is providing in more general terms (currently only 81 words)
- does not mention the improved Bayesian sampling mentioned in the introduction. This seems a key point of the paper.

Line 3 I think the abstract needs to mention that your "observations" are ERA5

Line 5 "Counterfactual world" - this is jumping in very deep very quickly into DA jargon

For the last three comments, we have reformulated the abstract.

Line 29 compbining

Done.

Lines 24-28 I would be helpful to have some non DA focussed literature on extremes in climate

Lines 29-34 very limited review of other literature attempting Bayesian approaches to present/future climate, see above

For the last two comments, we have added references.

Lines 34-35 Flow of text is a bit confusing. It is not clear the next paragraph is addressing these two issues because of the way it starts. The "Here," suggest the paragraph is setting off on a new topic.

Lines 46-47 Could you please compare like with like. Currently it is CPU time vs wall time. Surely wall time is primarily dependent on how much compute resource you have at your disposal.

Line 53 "observed" - not true observations should be something like "as represented in ERA5"

Lines 59,60 "classical attribution... specific definition" Not sure this will make sense to the reader

For the last four comments, the text has been reworded.

1.5.2 Data

Line 64: refer to refers

Done.

Line 69 being pedantic 0h to 23h misses out one hour but I know what you mean, perhaps 0:00 to 23:59?

We have provided the times at which the data is produced by ERA5 and the model, i.e., 00:00 and 23:00. It is corrected in the new text.

Line 74 Presumably three is some urban warming in the Paris observations. Please could you comment on how this affects the results somewhere, perhaps in section 6?

Indeed, three days correspond to the duration of the 2019 heatwave in France. In general, mortality increases sharply with the duration of heatwaves (D'Ippoliti et al., 2010), and a duration of three days allows us to capture this effect. Increasing the duration from a statistical point of view mainly amounts to reducing the sample size (the longer the heatwaves, the rarer they are), which limits inference. When choosing the variable, we added an explanation to this effect.

1.5.3 Method

Line 106 I am confused by the phrase "We add to the model". Are you suggesting $(X^R + X^G)$? If so, put it in the definition of eq 1. But if not perhaps "In addition, we can replace X^R with X^G.." or something similar would be clearer.

We meant that the model contains an additional element, which is the covariate $X^{\varepsilon} G \varepsilon$. We agree that this formulation might have been confusing. The entire Section 3 has been rewritten.

Line 111 English has gone a bit wrong here

The entire Section 3 has been rewritten.

Line 123 The notation for the scenarios is grim. Could we not have X^{R,A,S_i} and define S_i elsewhere?

We have changed the notations to make them easier to read.

Line 123 It would be good to acknowledge the unavoidable assumption that the climate system responds linearly to forcing. e.g μ_t and σ_t are constant wrt different forcing scenarios.

You probably meant that μ_0, μ_1, σ_0, σ_1 and ξ_0 are constants and do not depend on the scenario. This has been clarified in the entirely rewritten Section 3.

Lines 124-130 This assumes all GCMs are equal. It would be good to acknowledge this and that other approaches have seen it necessary not to make this assumption (references below)

The entire Section 3 has been rewritten.

Lines 131-132 this has been said earlier.

> Deleted.

Lines 143-144 it would be good to say that the energy balance model is forced with natural forcings only and the radiative forcings are natural only.

> It is now added in the revised version.

Line 151 θ^R and θ^R - second should be θ^G?

> Of course, corrected.

Line 171 Fig S3 seems to indicate σ_t is very very small. Worth commenting on the physical significance I think.

> You probably meant σ_1. This term is generally quite weak, although it can be stronger depending on the model. We discussed this in the new version, in Sect. 3.3.3.

Lines 171,200 Fig S3 and Fig 2c-f and their discussion. I think it would help the reader if you reminded them that these plots are site specific using your Paris observations

> It has been better highlighted.

Line 177 I think you can only say impossible if the probability of the shape being ≥ 0 is zero. I don't think you have yet shown this.

> Thanks you for this remark. The sentence has been removed.

Line 189 m is used as a superscript and a subscript in this line. Is this what you mean?

> Exactly, it is corrected.

Line 189 This is the first mention of bias between the climate models and truth which is very late in the paper. Too late. There should at least be a pointer earlier in the text that climate model bias is addressed later in the method description. Also I don't think you can deal with the issue of climate model bias in just 12 words! What sources of bias is it accounting for? In The GEV parameters? In the covariates? A description of how and how well is surely needed.

> Indeed. A discussion on model biases has been added when climate models are introduced.

Line 196 "grid point containing" earlier you were fitting to station obs for the plots (l137) - has it changed?

> Thank you for pointing this out. This was an error on our part, it was indeed the observation station. However, this has been changed, as we now only keep ERA5 in the revised version, due to some inhomogenity in the Meteo-France time series.

Line 196 Fig2b: 1940-1960 All GCMs are cool wrt Obs. Please comment.

This could possibly indicate that the number of degrees of freedom of the splines is a little too low to capture this part of the signal. A comment to this effect has been added.

Line 201 black ellipses missing

Sorry, this has been corrected.

Line 241 covariate FOR Europe

Thanks, corrected.

1.5.4 Comparison

Section 4 I'd suggest renaming to "Comparison with the independent scenarios method".

Thanks, but we have moved the section into the methodology section.

Line 275 What are the consequences of μ_1 being so different for obs vs gcm? Some would argue that if the climate model is so biased can we trust its physical representation of real world extremes.

In fact, μ_1 is wide in the observations because it is very poorly estimated: it depends solely on the trend, which has been significant over the last 30 to 40 years. This is why it is much better constrained in the models, since the scenarios allow 80 years of data to be added. A comment to this effect has been added.

Line 280 "Based on the estimates of the laws" - not sure this will mean very much to most people.

We have rephrased that to make it clearer.

Line 302 "average energy" - I wonder if this is the right term in a geophysical journal? Joules?

We have removed Wasserstein distances to clarify the presentation.

Line 304 I do wonder how much the average reader will get out of Fig 3 and I note that there is not that much discussion in the text for it. Perhaps just have the last column in a 2×2 format?

This section has been completely redesigned.

Line 314 "does a good job" - perhaps a bit too colloquial?

Yes, removed.

- Is there a low bias in ERA5 for some regions? eg UK had 40C in 2022 although this was only a single day. Kay et al. (2025) has much lower return periods for single day events and one would have thought that to first order changes in return period will be somewhat similar for different metrics of extreme hot temperatures.

This is entirely plausible, as in ERA5 the surface (i.e. the temperature at 2 m) is not explicitly resolved, but is an interpolation of the atmospheric reanalysis. Significant biases are observed, for example, in relation to E-OBS (see Fig. 1 at the end of the document).

- I think maps of GEV parameters would be very interesting to most readers, say for 2000, 2024, 2080? In the supplementary info if needs be.

The GEV parameter maps for the years 1850, 2000, 2024, and 2080 are now plotted for the four scenarios in figures 2-5 of the present answer document. We can see that the parameter μ_t evolves over time and is most sensitive to climate change. The parameter σ_t is almost constant (since $sigma_1$ is almost zero), while ξ_t is indeed constant over time. We do not believe that these figures add much value compared to the attribution in Section 6, which is why we did not include them in the revised version. Instead, we added a map of μ_0, μ_1, σ_0, σ_1, ξ_0 and the 1961/1990 anomaly of TX3x in the new Fig. 4 of the article, as well as a text commenting on them.

1.5.5 Example

Fig 4 is an odd beast. It seems like it is implicitly assuming that the climate has been stationary between 1940 and 2024. For example two points nearish to each other might have seen the same max temperature at very different times, say 1940 and 2024 for arguments sake. The probability of those two events are very different as the 2024 climate is much hotter. Yet the calculation of 4c assumes they have the same probability of occurring. At least could we have a complimentary plot of the 100 year return level (or whatever) with and without human influence and the difference please? Also for the caption I found "mean of all scenarios" a bit confusing as during the observed period the forcings are the same? I think it would be clearer to say with and without human influence.

This figure does not assume climate stationarity between 1940 and 2024. Three elements allows the construction of these figures:

- inference of the GEV distribution of maxima (over 3 days) at each grid point, which allows for climate non-stationarity;
- the definition of an event class as the maximum observed throughout the series;
- and finally, the calculation of the probability of this event class, which depends on the year (given that we are in a non-stationary context).

In this figure in particular, we calculate the return periods and intensity changes for the year 2024 specifically, but the inference of the law was indeed non-stationary.
It is true that the expression 'mean of all scenarios' can cause some confusion, so we have rephrased it, and we have added the details in the text.

Line 373 given the lack of spatial dependence perhaps a warning that these numbers cannot be used to calculate the likelihood of a hot event occurring in a given region or country without a correction to account for extremal dependence.

This warning has been added.

Lines 383-387 I find this spatial variation in return periods across quite small distances alarming. Some of this will be due to the issue in my point above but it does not seem physical.

You could check how the observed exceedance rates compare between different locations. e.g.

(a) Those areas where 4b shows very high return periods. What are the empirically observed exceedance rates? Are hot events occurring more frequently than predicted here?

(b) Also for the regions in 4b with very frequent return rates, N Africa, E Turky are we seeing their frequent occurrence in the observations?

(c) And eastern France and western Germany (approx. Nancy & Stuttgart) to see if the different return expectations from the plots in these two places are supported by the data.

(d) Kay et al. (2025) found the 2022 UK record event of exceeding 40C (admittedly a single day maximum, but one would expect different averaging periods to be somewhat in step) to be 1 in 24 years which seems rather at odds with your plot (4b) if >500y for the region where these temperatures occurred. Comment?

> It is true that these spatial variations appear to be very significant. In order to examine the situation in detail, we have plotted three grid points on Fig. 7 at the end of this document. One in Paris, the second in North Africa with a return period > 1000 years in 2024, and another nearby point with a return period < 10 years. We have represented the ERA5 series, as well as return periods of 2, 5, 10, 30, 50, 100 and 1000 years.
> It appears that the inferred return periods in North Africa are extremely close. What changes is the occurrence of a single extremely strong event (which is the maximum) that appears in one series and not in the other. This explains the spatial variations. We have also represented the GEV parameters in Fig. 6 (at the end of the document), which show no particular spatial inconsistency.
> In Kay et al. (2025), current observations at ground level and ERA5 (especially at maximum values) can be very different. As an example, we have shown in Fig. 1 (at the end of the document) the difference between ERA5 and EOBS (which is closer to the observations, but which we did not use because it is not available for the entire planet). We can see that biases of more than 1K to 2K are common (and up to more than 10K over North Africa), which explains potentially significant deviations from the work of Kay et al. (2025).
> A discussion about spatial variations and precautions regarding ERA5 has been added in the rewritten Sect. 5.1.

Line 385 I stumbled a bit over western and southern Asia. Perhaps "southern Caucasus" ?

> These names are not chosen by the authors, but are taken from standard M49 (UNSD, 2020).

Line 413 Don't think you need to repeat the results of the counterfactual world here as there is no reason for them to be different to 2040?

> That's right, we just mentioned that the conclusions were the same.

Line 404 these holes really bother me and I think they are an artifact of the methodology. Please can you diagnose their cause. Are they coming through the GEV terms or the X^* terms? Maps of all of these parameters at 2040 and 2100 (μ, σ, ξ, and all the X^S) would be helpful in this regard.

As mentioned in the answer above, the artifacts come from the existence of a particularly extreme event at certain grid points, which shifts the return times.

1.5.6 Conclusion

Section 7 seems to be an afterthought or the authors have run out of steam (not surprising). It would be good to have:

(a) A candid discussion of the limitations of the approach

Added.

(b) A more thorough comparison of results with other studies

Added.

(c) One of the difficulties this paper will have will be convincing people that the pain of understanding it and using the code is worth the effort. At present the case for is not very clear, at least to me. What might help is a companion plot of Fig S3 For Paris for a given return period say 1 in 100 years with uncertainty (ideally including a profile likelihood approach, see Coles 2001 Fig 2.3), for the method presented here VS a non stationary univariate GEV maximum likelihood approach with X_t calculated directly from the GCMs used.

The methodology section has been completely rewritten and much of the text has been reworked to make it easier to read and understand. We hope this helps.

Line 434 From my perspective very little verification has been undertaken with respect to current literature. Please could you explain why you think this statement is justified?

We had verified that the warming was consistent with the IPCC and the work of Ribes et al. (2021). We specified that we were referring to this particular calculation in the revised version.

Line 456 section 7.2 is rather niche and that there are far larger issues that would benefit having a discussion on. Such as how this work compares with other work that attempt to produce future posterior distributions of climate variables. I include references to such studies above.

We appreciate. We have added a discussion.

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Figures

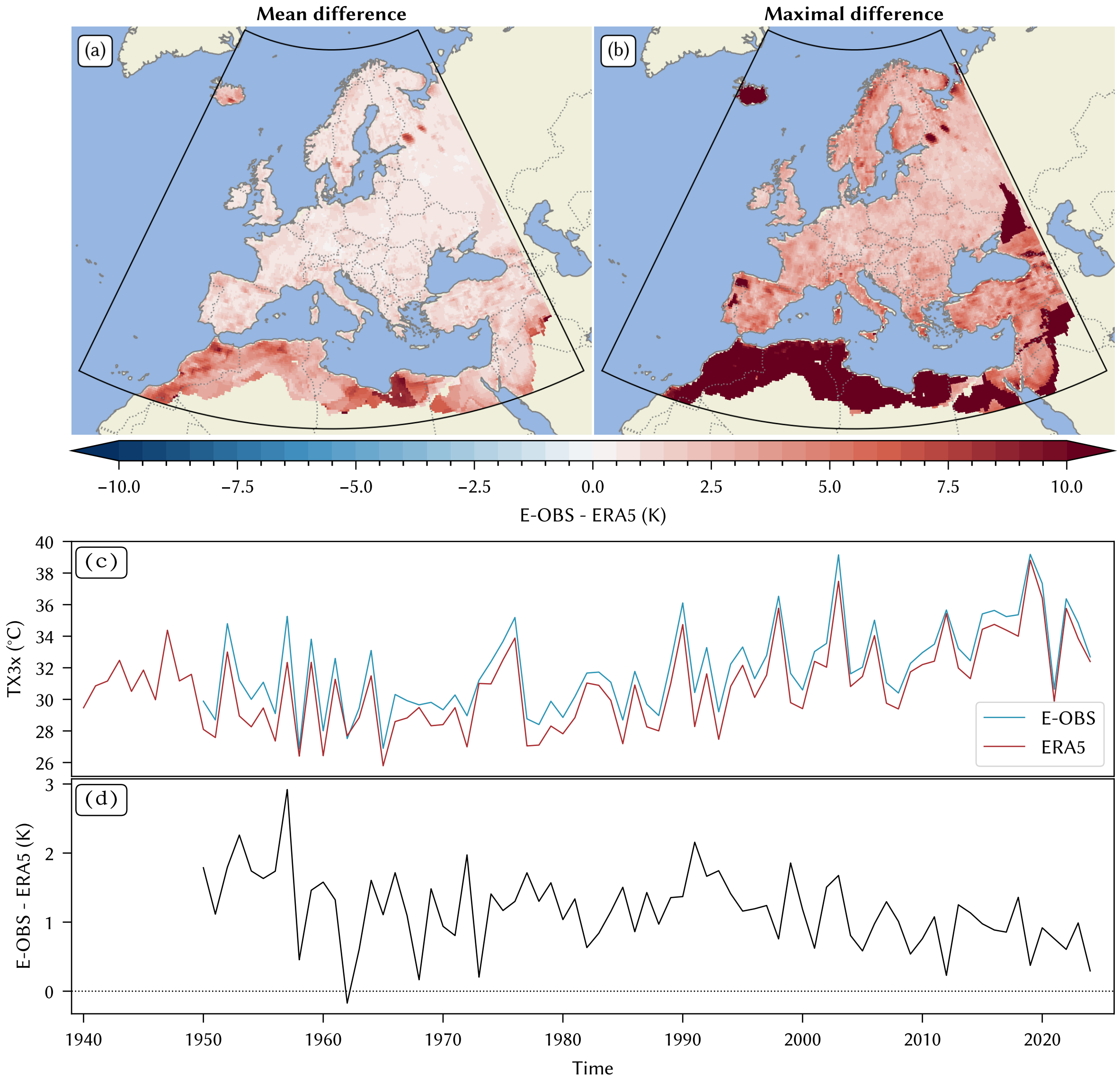

Figure 1: Difference between E-OBS (Cornes et al., 2018) and ERA5 (Hersbach et al., 2020) for the TX3x variable over Europe. **a)** Average difference over the period 1940–2024. **b)** Maximum difference over the period 1940–2024. **c)** TX3x series from E-OBS (blue) and ERA5 (red) in Paris. **d)** Difference between E-OBS and ERA5 in Paris between 1950 and 2024.

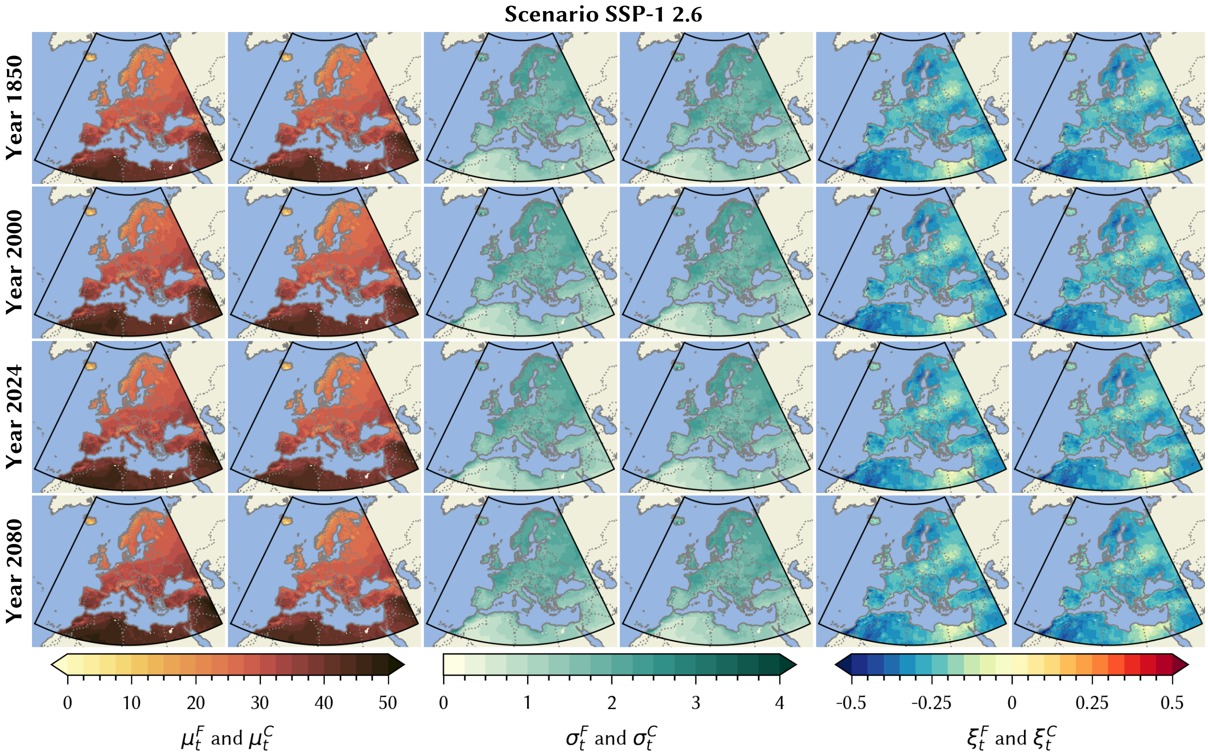

Figure 2: Map of the different parameters μ_t, σ_t and ξ_t in Factual and Counter-factual world of the GEV model after observational constraints, for the years $t \in \{1850, 2000, 2024, 2080\}$.

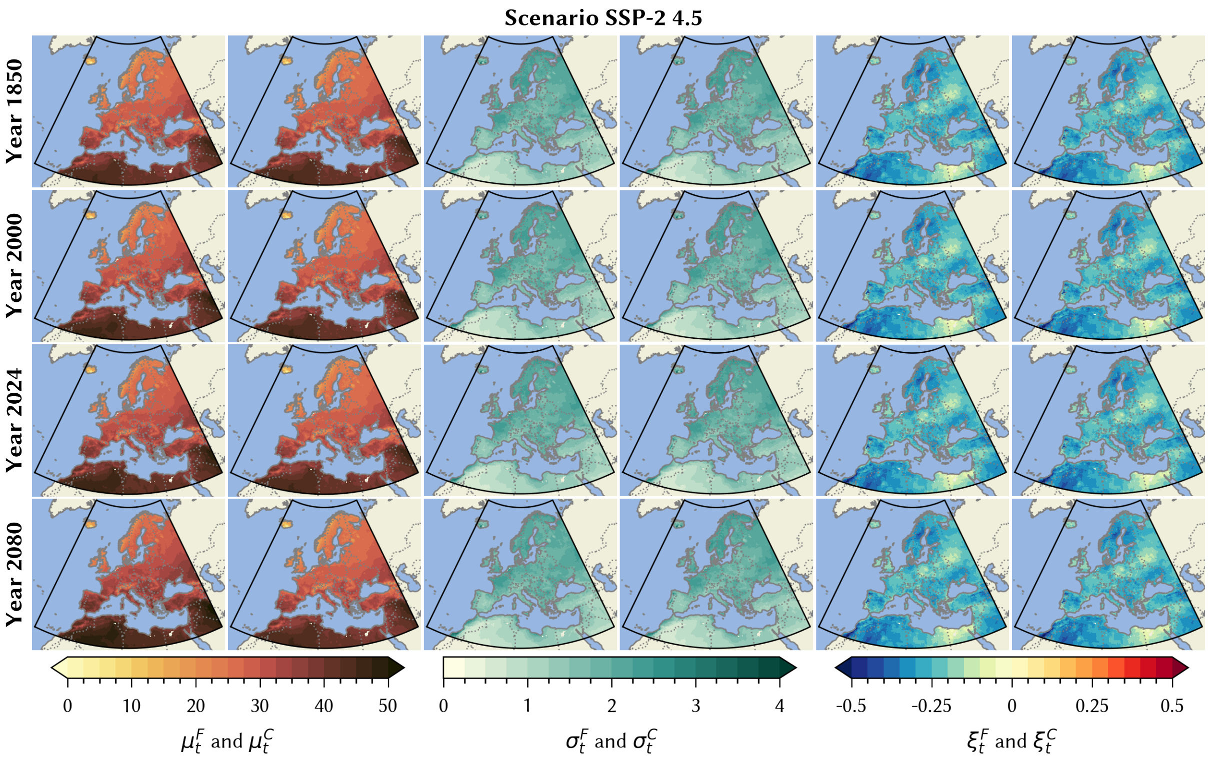

Figure 3: Map of the different parameters μ_t, σ_t and ξ_t in Factual and Counter-factual world of the GEV model after observational constraints, for the years $t \in \{1850, 2000, 2024, 2080\}$.

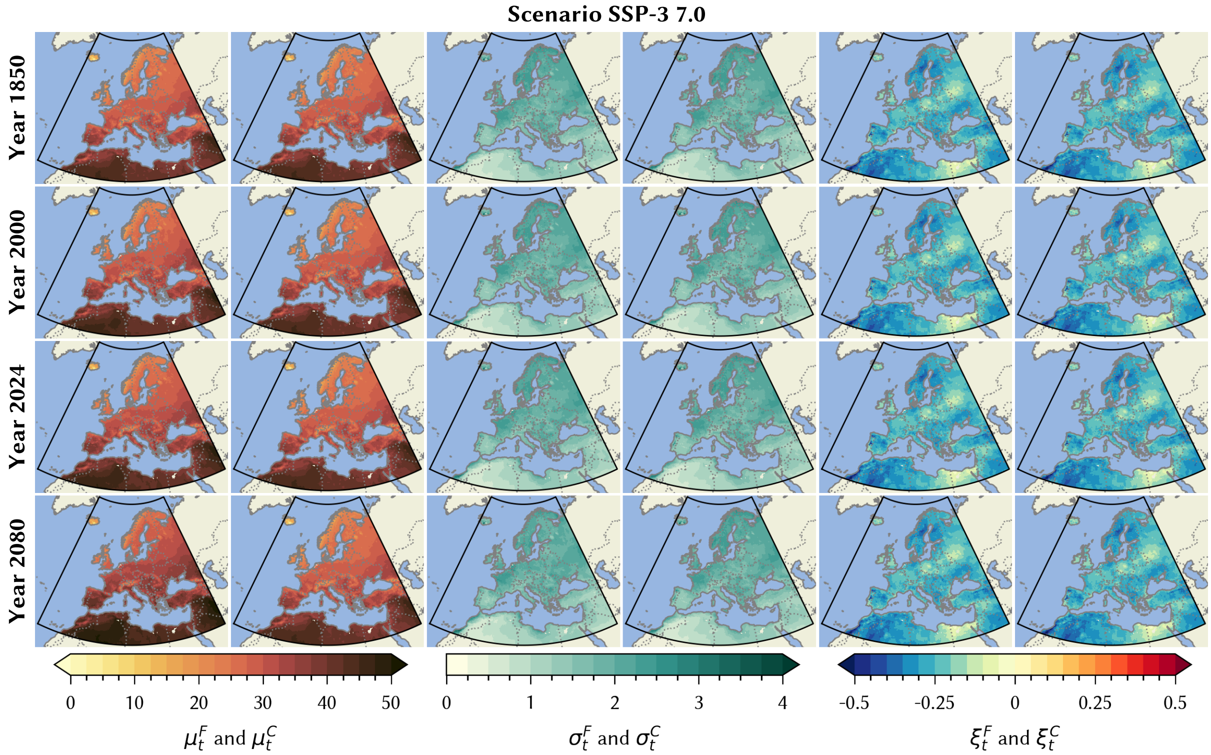

Figure 4: Map of the different parameters μ_t, σ_t and ξ_t in Factual and Counter-factual world of the GEV model after observational constraints, for the years $t \in \{1850, 2000, 2024, 2080\}$.

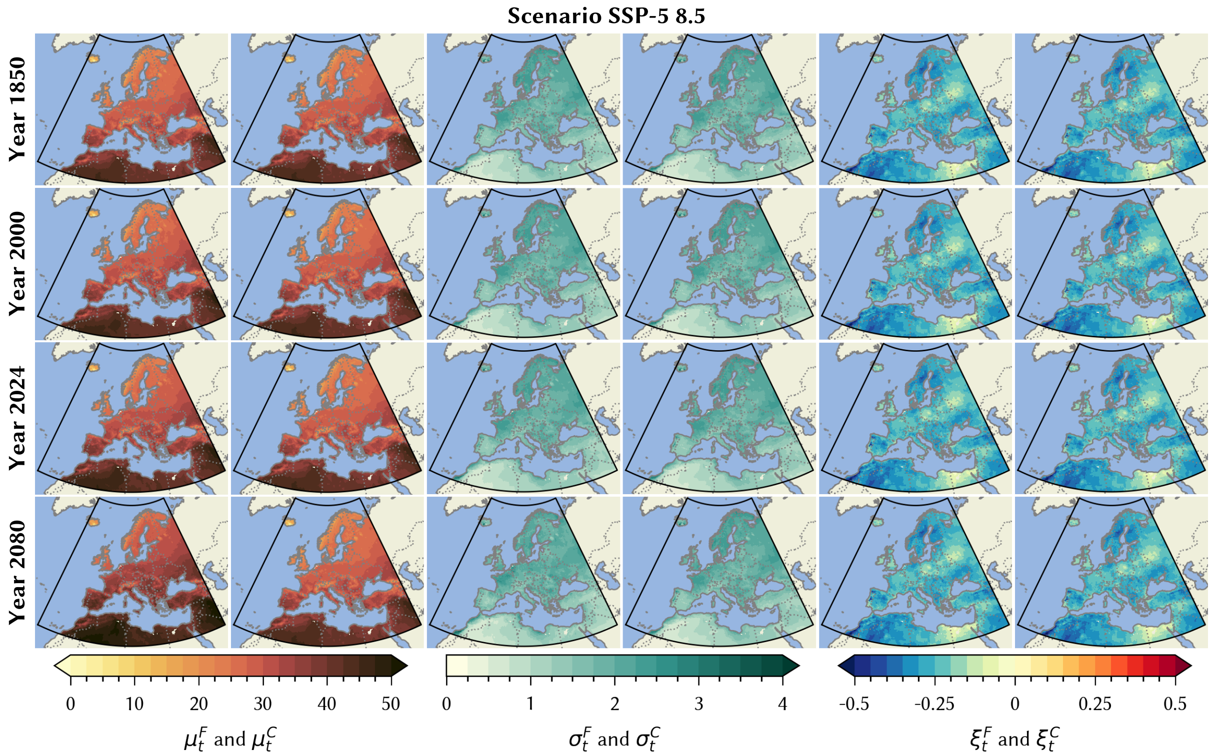

Figure 5: Map of the different parameters μ_t, σ_t and ξ_t in Factual and Counter-factual world of the GEV model after observational constraints, for the years $t \in \{1850, 2000, 2024, 2080\}$.

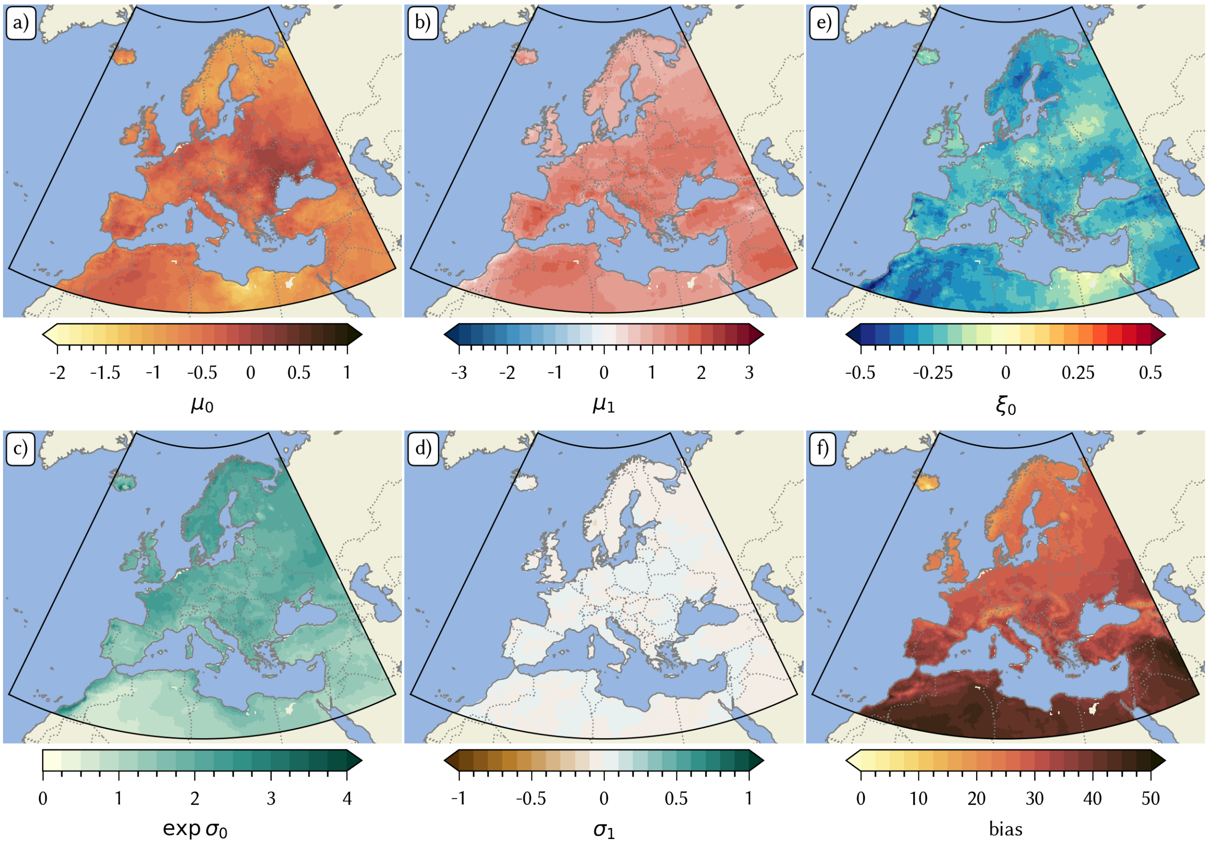

Figure 6: Map of the different parameters of the GEV model after observational constraints. **a)** Constant of the location parameter μ_0. **b)** Trend of the location parameter μ_1. **c)** Constant of the scale parameter $\exp(\sigma_0)$. **d)** Trend of the scale parameter σ_1. **e)** Constant of the shape parameter ξ_0. **e)** Bias of TX3x from ERA5 (mean over 1961 / 1990).

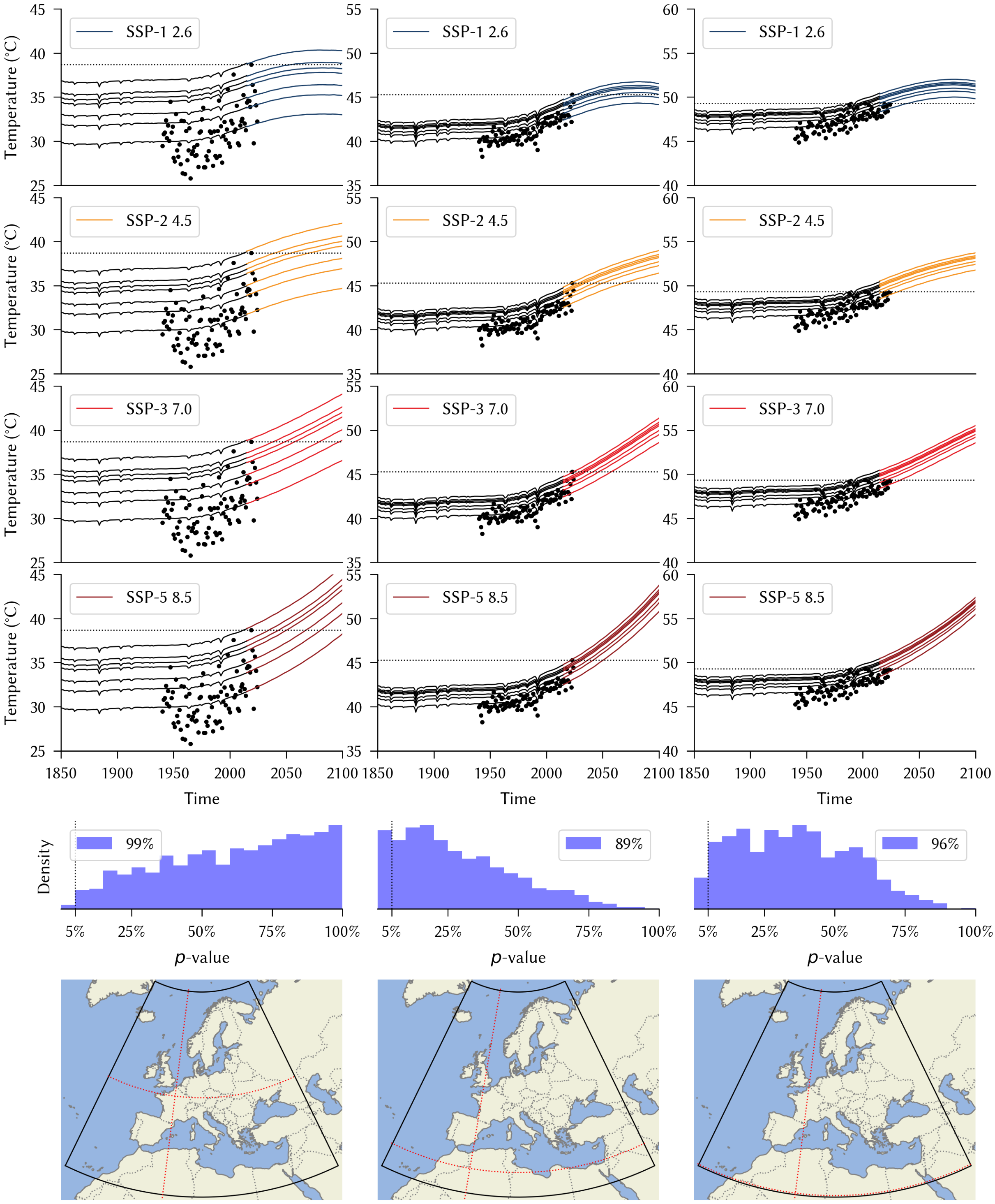

Figure 7: Comparison between observations and the inferred GEV distribution for three grid points (one per column). The position of the grid point is shown on the map (last row). The grid points are chosen, in order, in Paris, at a point where the maximum has a return period > 1000 years in 2024, at a point where the maximum has a return period < 10 years in 2024. The first 4 lines (representing, in order, the 4 scenarios SSP1-2.6 to SSP5-8.5) show ERA5 (black dots), the maximum value of ERA5 (black dotted line), as well as the following return levels: 2, 5, 10, 30, 50, 100, and 1000 years. Note that the scale is chosen to be comparable between the three columns (spread of 20°C). The fifth line shows the histogram of the p-values of the KS-test of 1000 samples compared to ERA5. The probability indicates the number of tests where the p-value is greater than 5% (threshold where we do not reject that the observations follow the inferred GEV law).