

Clim. Past Discuss., author comment AC2
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Reply on RC2

Chantal Camenisch et al.

Author comment on "A Bayesian approach to historical climatology for the Burgundian Low Countries in the 15th century" by Chantal Camenisch et al., Clim. Past Discuss.,
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Reply to anonymous referee #2

In their manuscript, Camenisch et al. present a new approach to quantitative reconstruction of temperature and precipitation characteristics from documentary sources. Through Bayesian inference, categorical data derived from historical archives are assimilated into GCM-generated ensembles of climate simulations, effectively combining temporal variability of both these sources. Application of the technique is demonstrated for seasonal temperatures and numbers of days with precipitation in the Low Countries (NW Europe), over the 1420-1499 CE period.

The paper is competently written and topically well suited for the 'International methods and comparisons in climate reconstruction and impacts from archives of societies' special issue of the 'Climate of the Past' journal. I only have a few comments/suggestions regarding the methodology, results, and their presentation (I leave it at authors' discretion whether and how they will consider them in preparation of the final manuscript):

Many thanks for your referee report and your comments!

(C1) Extensive ensembles of GCM simulations were used to generate the base (prior) probability distributions. However, since the year-to-year variability in such simulations is largely uncorrelated with historical variability in the climate system, retaining full intra-ensemble variability (as described in Sect 3.3) seems to add unnecessary noise to the prior data. This noise is then partly carried over to the posterior data (this is especially apparent in Fig. S1, visualizing results obtained for the smaller (13-member) CESM-LME ensemble). Perhaps using somewhat less 'noisy' data to generate the prior probability distribution (e.g. by employing mean value of the ensemble instead of its complete spread) would result in less noisy reconstructions, while still retaining the relevant variability from the GCM-simulated series (such as components tied to boundary conditions and external forcings, which are shared by all ensemble members).

Many thanks for this comment. We understand that GCM internal variability has usually a low correlation with historical variability. However, climate simulations are physically consistent, and therefore they are well suited to describe the background state of the atmosphere prior to any observation. For instance, GCM outputs are especially good at capturing the climate response to external forcings such as volcanic eruptions, which can

further improve the reconstruction of post-eruption years. In this sense, a large ensemble of GCM simulations is needed to calculate the prior probability distribution. Note that if only the mean value of the ensemble is used, the prior probability distribution of Pfister indices would only have one value with a likelihood of 1. Moreover, the historical variability is embedded into the posterior probability distribution through the information provided by historians, yielding high correlations among reconstructions with priors generated from independent GCM ensembles (HIPPO vs CESM-LME) with completely different internal variabilities (Table 4).

(C2) Minimum probability threshold of 0.05 was prescribed when generating the probability distributions (l. 154+). It feels that in some situations, this may act as unnecessary artificial degradation of the signal (e.g., when a distinctly hot summer is indicated by the documentary sources, yet the probabilities for sub-normal temperatures are still set to be greater than zero regardless). Perhaps using a simple formal parametric approximation of the probability function, e.g. by (suitably transformed) binomial or Gaussian distribution, would better capture the related uncertainties (with probability values outside of the most likely categories still being non-zero, but not constrained by an arbitrary constant).

The initial idea was not to include any threshold to the observational likelihoods, as suggested by the reviewer. However, this led to a posterior probability distribution highly governed by the observations, removing most of the prior information provided by climate models. Note that, for observational likelihoods close to 0, the posterior probability is also close to 0, regardless of the prior probability. Therefore, a consensus between historians and climate scientists was achieved to set a minimum threshold of 0.05. The 0.05 is based on the experience with the relevant historical sources, and not only statistical convenience. Sources of the period may contain copying errors and incorrect dates. Thus, even for a cold summer, we might have a description of great heat: not because observers were incapable of telling hot from cold, but because we have a description for the wrong year or location. And it allows for the partial propagation of information from model outputs into the posterior probability distribution. In future studies, we may consider whether the correct likelihood should be more like 0.2 or 0.3, but we chose to err of the side of caution and simplicity in this first study. We will clarify this point in the revised version of the manuscript.

(C3) I wonder about uncertainties/ranges shown for the reconstructions in Figs. 6 & 8 and how they relate to the posterior data visualized in Figs. 4 & 5. For instance, in Fig. 4a (winter temperature), the 1459 CE temperature estimate seems quite uncertain (i.e., the posterior probability distribution is rather widely spread among several categories), whereas much lower uncertainty is indicated for 1460 CE (narrower probability distribution, dominated by a single category). Yet, there is no major difference in the size of the estimated temperature ranges for these years in Fig. 6 (in fact, the ranges seem to be near-identical in size throughout the entire period covered). If these are derived solely from min-max values of the GCM ensemble (as described at l. 233+), perhaps it would be useful to also provide uncertainties derived from the spread of posterior distributions in their entirety (and thus to consider not only uncertainty of the prior (GCM-based) data, but also that from the documentary sources).

The reviewer is right and uncertainties derived from the spread of posterior distributions will be included in Figs. 6 & 8. The same way that seasonal mean temperatures were calculated using the posterior probability distribution of Pfister indices, their corresponding weighted standard deviations will also be calculated, so that reconstruction uncertainties can be better assessed.

(C4) It might be useful to see how well the temperature/precipitation reconstructions match actual weather variability typical for the target region (to see if, e.g., variance of

the reconstructions matches the real climate, or if there is under/overestimation). This could be done, for instance, by adding observational distributions for the instrumental period to Figs. 7 and 9 (and discussing which eventual differences stem from comparing two different periods, and which may be related to biases in the reconstruction itself).

This is a good suggestion that can be implemented in the revised version of the manuscript. Although a direct comparison of the 15th century against the 19th-21st centuries cannot be made, an observational dataset such as HadCRUT 5 / CRUTEM 5 (land only) can be used to obtain an observational distribution of the temperature since 1850 CE, allowing for the analysis of temperature changes between those two time periods. On the other hand, a more challenging task is to find an observational data set of wet days in a month over the area of interest, and therefore we will have to rely on reanalysis for the precipitation assessment, which can be biased due to the model component of these hybrid products.

Minor/technical comments

Abstract, l. 9+: ‘... our reconstructions present a high seasonal temperature correlation of $\square 8$ independently of the climate model employed to estimate the background state of the atmosphere.’ – it is not quite clear from this formulation what the correlation value refers to (i.e., which two signals are being compared)

We will clarify this in the revision.

- 188: ‘... (drier) conditions are associated with positive indices.’ – this seems to clash with definition at l. 131, which associates positive values of the index with wetter conditions

We will correct this.

- 198: comma instead of dot

We will correct this.

Fig. 9: Maybe it would be useful to add a symbol to each of the four post-volcanic years (instead of just number), so that it is more clear that these are specific data points

We will clarify this in the revision.

Sect. 2: Perhaps elaborate a bit more on the exact extent of the target region – it might be particularly helpful to show locations pertaining to individual documentary records used, e.g. by including them in Fig. 1

See reply to referee 1.