**Response to Anonymous Referee #3**

We thank Referee #3 for reading the manuscript carefully and providing thoughtful and constructive comments. The comments are noted with RC, our responses with AC, and the intended additions or changes in the manuscript are underlined.

This is a very well written paper (with high quality figures) on discharge simulations based on climate model outputs, with "a typical simulations chain" (abstract first line). The paper was easy to read, which is a great achievement for such a complex modelling paper.

We thank Referee #3 for his/her encouraging general comments.

**RC3.1** The topic is probably undersold: it is not just a case study but one of the few attempts to produce long simulations of the past, with a high number of challenges to overcome. I am not aware of long simulation studies back in the past in mountainous environments with climate model outputs (I might not be on top of that literature but I suspect there are very few).

Accordingly, the added value to the literature should be formulated in a much more straightforward way as early as possible in the introduction and again in the conclusion.

The method is not well captured in the abstract or in the paper overall and calling this a "typical simulation chain" is perhaps underselling. A critical step in such simulation chains is how to combine reanalysis data with a hydrological model, especially in mountainous areas. What does this study do differently than other works? Why is the approach more interesting?

> **AC3.1** Thank you for this comment. This point was also highlighted by Referee #1 (RC1.1) and Referee #4 (RC4.1).
>
> As mentioned in the introduction, a large number of previous studies described downscaling approaches to adapt climate model outputs used to force hydrological models. Several studies highlighted that the choice of the downscaling method can strongly modulate the hydrological regime simulated with the same model (Wood et al., 2004; Quintana Seguí et al., 2010; Chen et al., 2011).
>
> Realistic hydrological simulations based on a specific downscaling approach and fine model calibration at the basin-scale are often used for relatively short hydrological simulations, lasting from one year to one decade (e.g. Habets et al., 1999; Boscarello et al., 2014), while longer periods are required to study trends in precipitation (Ménégoz et al., 2020) and river flows (Brönnimann et al., 2022).
>
> Bonnet et al. (2017) simulated the water cycle at the scale of the entire French hydrological system over the 20th century, a period long enough to discuss decadal variability and long-term trends. However, their model shows local deficiencies, particularly in mountainous areas where the snow cover plays a crucial role, and where the dams - not considered in their study - have significantly affected the river flows since the 1950s.
>
> The novelty of our study relies on three combined features: (i) we used a fine model calibration of the hydrological model to accurately simulate snowmelt, including the impacts of the dams

at the scale of the Upper Rhône River catchment; (ii) we applied this model over a long enough period - the entire 20th century - to allow a complete evaluation of the model to simulate the hydrological regime in terms of mean, variability, long-term trend and extremes; (iii) we compared two configurations, one based on statistical downscaling and the other one using dynamical downscaling, highlighting the need to accurately simulate the temperature and precipitation lapse rates. Overall, we confirm the need for an additional bias correction of atmospheric variables after the downscaling step and before the application of the hydrological model.

The novelty of our study will be deeply described in the revised version of the article.

**RC3.2** A concise summary of the method would help (take a hydro model, calibrate on observed meteo and streamflow data, run with meteo scenarios generated based on reanalysis data; contrary to many other works, no simple spatial downscaling of reanalysis data but use of a weather generator to produce mean areal precipitation scenarios).

**AC3.2** Thank you for this suggestion. In the first version of the article, each step of the model chain was described, but a general description of the method was clearly missing.

We will include in the revised version of the article, at the beginning of the section devoted to the methods, a synthetic summary of the model chains.

Details:

**RC3.3** Method: it should be made very clear at what scale MAP (mean areal precip) and MAT (temperature) are defined. I see from the sentence "bucket-type model that uses time series of Mean Areal Precipitation (MAP) and Temperature (MAT) as inputs for each hydrological unit." that MAP and MAT is defined at the elevation band scale (?)

**AC3.3** Yes, MAP and MAT are defined at the scale of elevation bands. The MAP and MAT time series are different from one elevation band to another, due to the different elevations and Thiessen weights for the neighboring stations. This point will be clarified.

Note that MAP and MAT are also estimated for other spatial scales. In our evaluations, we compare simulated and observed MAP and MAT at the scale of the 5 major sub-basins shown in Fig. 1. We will specify the spatial scale at which the different MAP and MAT are considered in each section.

**RC3.4** "a daily potential evapotranspiration time series is derived from temperature", how? Linear?

**AC3.4** To develop our T-PET model, we use monthly CRU values of PET and temperature T at a resolution of 0.5° x 0.5° for the 1900-2010 period. PET is assumed to be a linear function of temperature T: $PET(t) = a \times T(t) + b$. The coefficient b depends on the calendar month. This model, estimated for the entire Upper Rhône River catchment, is then used at a daily time step to produce daily PET for all its hydrological units.

This point will be specified in the article.

**RC3.5** "For each RHHU, daily MAP and MAT are estimated from neighboring weather stations using the Thiessen's weighting", why Thiessen? Probably very inappropriate for mountain environments? And how is the Thiessen obtained between points (weather station locations) and elevation bands?

**AC3.5** As shown on the two maps in Fig. A, the Thiessen polygons are derived from the network of available stations in the region (within the catchment and in its neighborhood).

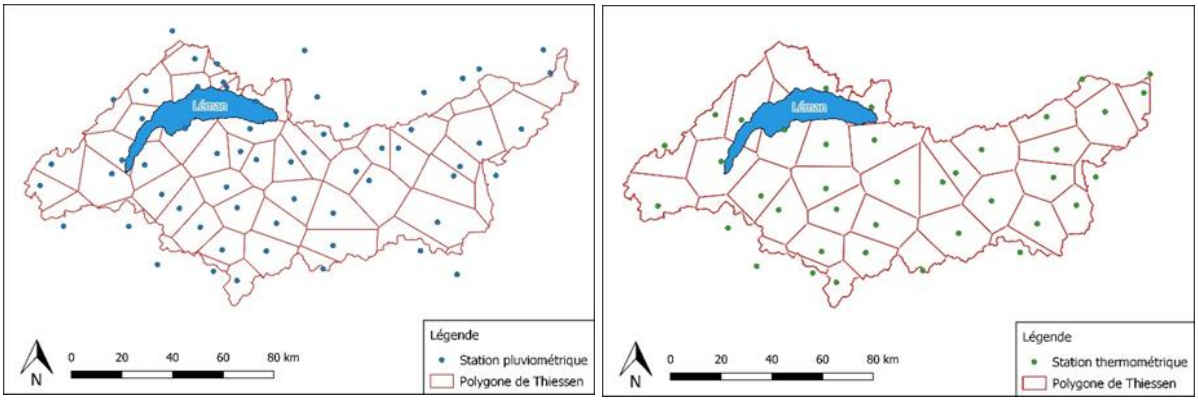



Figure A. Thiessen polygons for (left) the precipitation stations and (right) the temperature stations considered for the study.

The Thiessen weights of the different stations for any given surface area are estimated as follows. Let's call Ai, the surface area of the considered unit covered by the ith station. For most stations, Ai will be zero. The weight associated to station i is simply Ai / A where A is the surface area of the considered spatial unit. Whatever the spatial unit (elevation band, sub-basin, URR basin, …), the sum of the weights is always equal to one for this unit. The weights are always different from one spatial unit to another, from one elevation band to another, depending on the proximity of the stations to that unit.

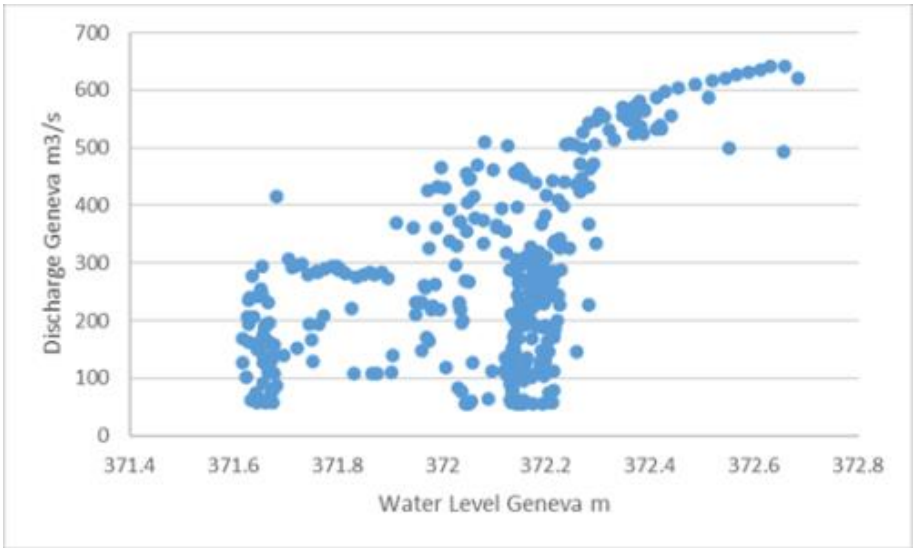
Note that the URR basin is divided into 18 sub-basins. For each of them, we always have 5 to 10 stations for which the weights are non-zero for precipitation. If we disregard the issue of elevation dependency, which is widely discussed in the article, we considered that this number provides a reasonable estimate of MAP for these areas. Note that this issue of elevation dependency would not be better addressed by other spatialisation approaches (inverse distance, kriging).

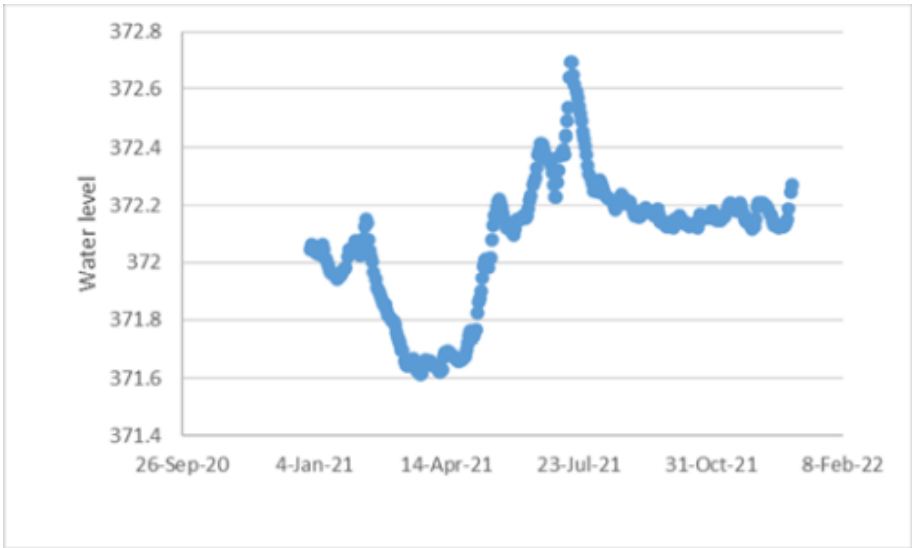
For temperature, the number of stations with non-zero weights is lower, but this variable, when considered at a given reference elevation, varies much less spatially. The way temperature is estimated for each unit is as follows: apply a weighted Thiessen sum of station temperatures, corrected in a previous step to correspond to the reference elevation for that unit (thanks to the temperature lapse rate discussed in the article). For most time steps, the linear temperature-elevation relationship is very robust, allowing a very reasonable temperature estimate for altitudes different from those of the stations.

**RC3.6** What is the storage equation in this model? Is it the storage-volume relationship? Where do you get if from?

**AC3.6** This is the water level-storage equation mentioned in the next sentence. This point will be clarified.

**RC3.7** How do you construct the linear outflow relationship? As far as I see from one year of observed data (extract from the pdfs https://www.hydrodaten.admin.ch/en/seen-und-fluesse/stations/2028 and https://www.hydrodaten.admin.ch/en/seen-und-fluesse/stations/2606) any outflow is possible for low water levels (see figures below) and a linear relationship only holds for high water levels and flow beyond 500 m3/s




**AC3.7** We could not find these figures on the above-mentioned websites. However, the data used for these figures are likely those of the last few years, decades when a regulation applies. The water level-outflow relationship has thus no reason to be univocal, since the operations for a given day (hour) must be defined according to the state of electricity demand and electricity prices for that day (hour). An exception is made for high water levels, when operations aim to limit the water level in the lake to avoid riparian flooding. In this case, the only decision variable for operation is the lake water level, which explains the univocal relationship found in the data.

If the lake were unregulated, we would probably have a univocal relationship in first approximation, due to its very large surface area and the topographical configuration of its outlet. The discharge would be a function of the hydraulic head. As the water level does not vary a lot in the lake (much less than 2 m), we considered the relationship was to be linear.

**RC3.8** Can you say something about the evaporation equation, what does it include? I have not seen the Rohwer's equation (Rohwer, 1931) before.

**AC3.8** We used Rohwer's equation (see equation below) as it uses an additional variable, namely atmospheric pressure at 10 m, and this was found of potential interest for further use of the model in other climate contexts. Note that Rohwer's equation is very close to the better-known Penman (1948) equation (see below), and that the difference of the mean annual evaporated water over the 1961-2010 period between Rohwer's and Penman's estimates is only 3%. This choice is thus expected to have little impact on the main conclusions of our study.

Rohwer (1931) equation:
E(t) = 0.77 x (1.465−0.0186 x p_10m(t)) x (0.44+0.118 x U_10m(t)) x (es_surf(t)-ea_10m(t))
where :

- E(t) (in j-1) is the evaporation from Lake Geneva at time step t.
- p_10m(t) (inHg) is the atmospheric pressure at 10 m at time step t.
- U_10m(t) (mph) is the wind speed at 10 m at time step t.
- es_surf(t) (inHg) is the vapor pressure of saturated air at lake surface temperature T_surf(t) at time step t.
- ea_10m (t) (inHg) is the current vapor pressure in the air at 10 m at time step t.

Penman (1948) equation:
E(t) = 0.35 x (1+0.24 x U_10m(t)) x (es_surf(t)-ea_10m(t))

**RC3.9** "In the present work, SCAMP was used to generate 30 time series of daily spatial weather scenarios for the 1902-2009 period from ERA-20C reanalysis outputs." At what spatial scale? At the scale of the 7 km x 7km of the MAR model? This would be in contradiction with Fig. 3 that says that you generate MAP (mean areal precip) estimates and not first spatial estimates? So, do you first produce spatial estimates, i.e. daily time series per pixel? If yes, how do you combine the pixels into MAP estimates? By taking the pixels within the elevation band? Does this make any sense?

**AC3.9** Thank you for this comment. There was indeed a mistake in Fig. 5, where disaggregated data are shown for MAR and SCAMP. SCAMP indeed produces precipitation and temperature time series at station level, and the stations at the end of the process are exactly the same as the observation stations.

<u>Fig. 5 will be corrected: the spatial resolution of P/T is "stations'' for obs and SCAMP, and "7x7 km" for MAR</u>.

Sorry for this. We understand it was almost impossible to understand the spatial aggregation/disaggregation process. We hope it is now clear.

**RC3.10** Method overall: how do you have confidence that the conceptual model calibrated on observed streamflow data with observed station meteo does a good job if used with weather generator scenarios produced at a different scale? What part of your method ensures this?

**AC3.10** Thank you for this comment. The conceptual hydrological model is mainly used here as a complex and non-linear filter to assess the weather scenarios. Note that the model can be imperfect as we also use it to produce the hydrological reference - against which hydrological scenarios derived from weather scenarios will be compared. The hydrological model is used as a filter to assess the relevance of the downscaling models.

As mentioned in our article, our downscaling models are forced with time series of large-scale atmospheric circulation. Perfect downscaling models would allow to perfectly reproduce the multi-scale spatio-temporal dynamics of weather over the time period used for the simulation. Downscaling models can be assessed, at several scales, for their ability to reproduce observed spatio-temporal dynamics of weather variables. This assessment is rather difficult, especially with regard to the covariability between weather conditions in different sub-basins of the Upper Rhône River basin, or with regard to the covariability between precipitation and temperature.

The hydrological filter is a powerful alternative filter for making this assessment, as the hydrological behavior of sub-basins is highly non-linear, and depends on all the covariability and dependency structures of meteorological conditions, both spatially and between variables. In short, we need to assess the meteorological variability and covariability features that are important for hydrology and the hydrological model does this job well.

**RC3.11** Model calibration on signatures: did you check for the catchments with observed concomitant streamflow if the signature calibration gives good results?

**AC3.11** Thank you for this comment. To assess the relevance of the signature-based calibration, we carried out the following experiment. We recalibrated the parameters of sub-basins with a natural (or at least not significantly altered) hydrological regime, using only the hydrological signatures (Fig. S5).

We then examined the time series obtained with this signature-based calibration. The simulated time series remain in very good agreement with the observed flows (Fig. S6). The NSE coefficients are logically lower than those obtained with the classical calibration approach, but the difference is quite small (Fig. S6).

<u>We will add comments on these analyses in the article and provide Fig. S5 and S6 in Supplementary Materials.</u>

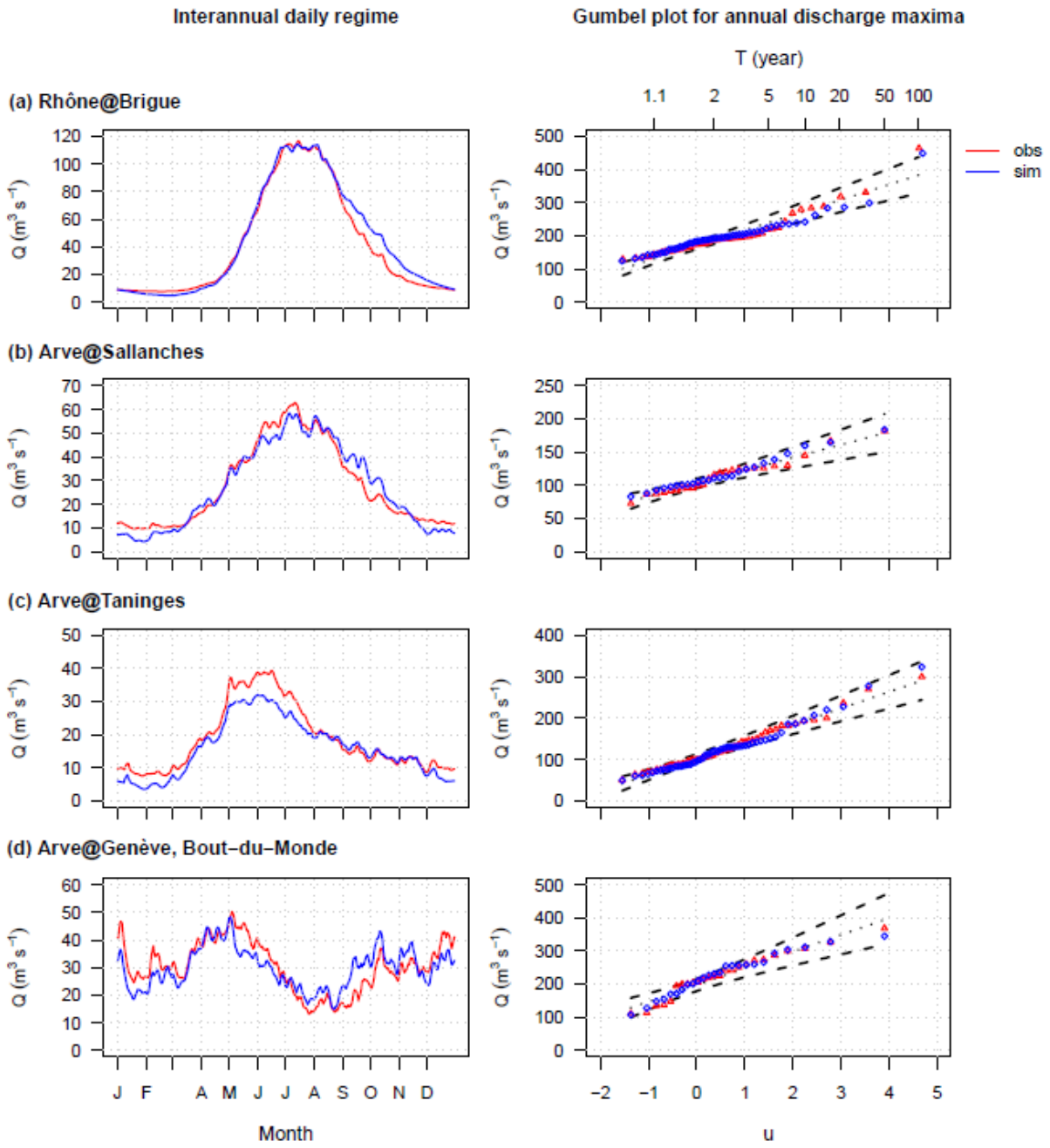



Figure S5. Signatures-based calibration of sub-basins with a natural (or at least not significantly altered) hydrological regime. (a) Rhône@Brigue (1965-2015). (b) Arve@Sallanches (25 years of non-continuous data between 1965 and 2015). (c) Arve@Taninges (1961-2015). (d) Arve@Genève, Bout-du-Monde (25 years of non-continuous data between 1965 and 2015). (left) Interannual daily regime. (right) Gumbel plot for annual discharge maxima. The x-axis is the reduced variate u for the given return period T, i.e. u = -ln(-ln(1-1/T)). Dashed lines correspond to 90 % confidence bounds of the Gumbel distribution estimated on observed data. The calibration criteria are the NSE coefficient between simulated and observed regimes and the Kolmogorov-Smirnov coefficient between the observed and the simulated distributions of annual discharge maxima.

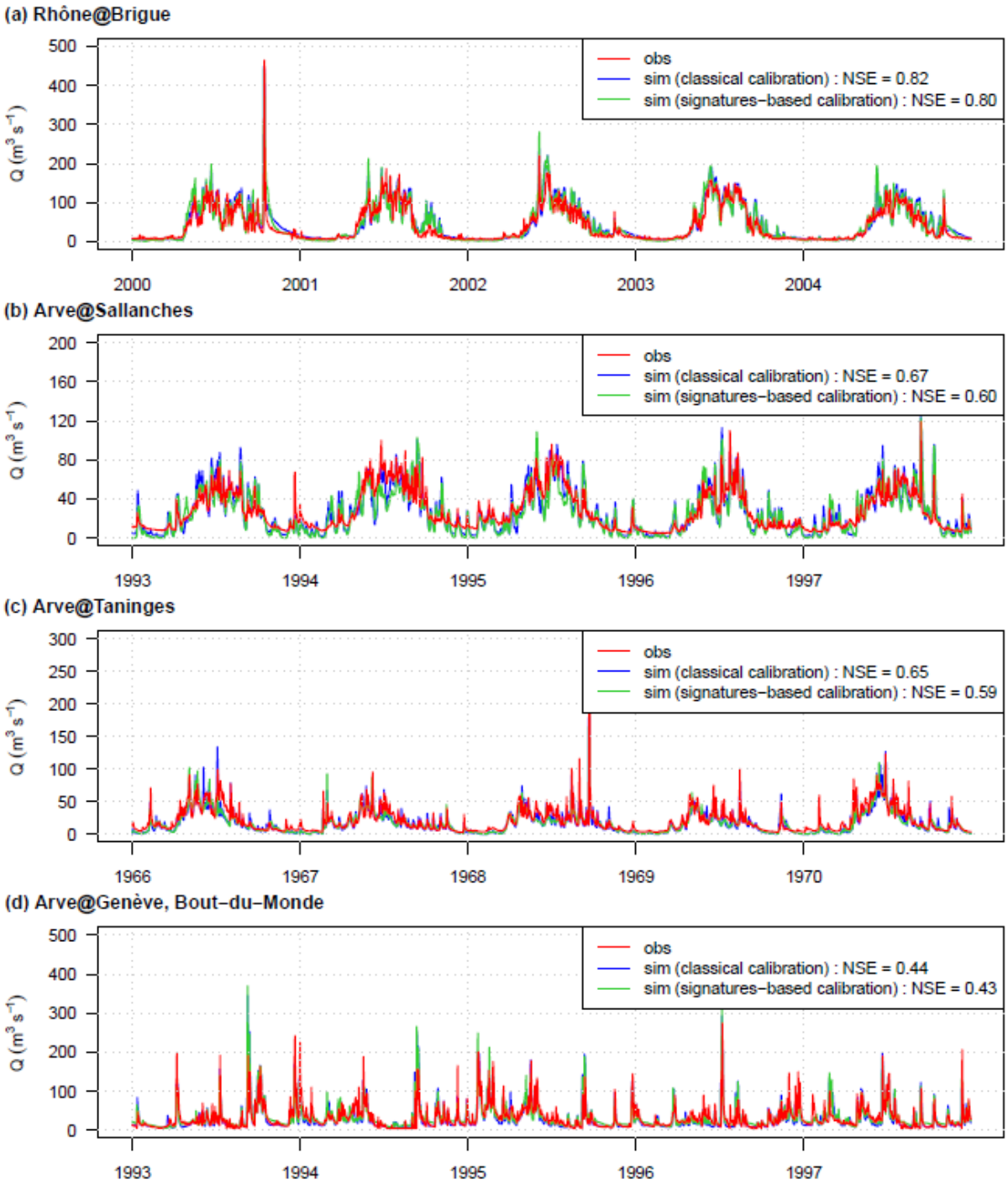



Figure S6. Comparison of the two types of calibration for sub-basins with a natural (or at least not significantly altered) hydrological regime. NSE values are shown for both calibration types: classical calibration and signatures-based calibration. (a) Rhône@Brigue. (b) Arve@Sallanches. (c) Arve@Taninges. (c) Arve@Genève, Bout-du-Monde.

Bias correction

**RC3.12** Do I understand correctly that bias correction is done independently for temperature and precipitation, which are also downscaled independently. How do you ensure a good link between the two for the simulation of snow accumulation and melt?

> **AC3.12** Thank you for this comment. Yes, the bias correction is performed independently for temperature and precipitation. The downscaling also appears to be performed independently, but this is not exactly the case. As the downscaling is conditioned each day by the atmospheric conditions of the day, some dependency between all variables (in space and between temperature and precipitation) is expected to be introduced, as shown by Mezghani and Hingray (2009).
>
> In this work, also applied to the Upper Rhône River catchment, the downscaling model was evaluated in terms of its ability to reproduce - for different spatial and temporal scales - different observed weather statistics: namely, observed MAP, observed MAT and "observed" Mean Areal Liquid Precipitation. The latter depends entirely on the variables MAP and MAT, as MAT defines the nature of precipitation. Mezghani and Hingray (2009) show in Fig. 13 that Mean Areal Liquid Precipitation statistics are well reproduced whatever the catchment. This suggested that a relevant link between precipitation and temperature is indeed derived thanks to the conditioning of the weather generator to large-scale information.
>
> Due to lack of space, we will try to add a short comment on this point.

Results

**RC3.13** In the MAT series, we seem to see nicely the 1980 global regime shift (Reid et al., 2015), with a sharp increase of temperatures, perhaps worth commenting?

> **AC3.13** The statement describing this shift will be adapted as follows:
>
> "The positive trend in temperature starting in 1980 is also adequately reproduced." replaced by "The positive trend in temperature starting in 1980 is also adequately reproduced, this resulting from the global combination of the warming related to anthropogenic GhGs and the reduced anthropogenic aerosol cooling (Reid et al., 2016), especially pronounced over Europe (Nabat et al., 2014)."

**RC3.14** Figure 11c would certainly contain some very interesting information but I cannot see anything. I would split into two figures, and rescale to 200 and 500 m3/s; furthermore, you could perhaps some coefficient of variation and autocorrelation at lag 1 instead of simply the figure?

> **AC3.14** We acknowledge that the figure was difficult to read. We will split it into two figures (see below). Fig. 11c concerns simulations with bias-corrected scenarios and Fig. 11d concerns simulations with raw scenarios. The better agreement with observations when bias-corrected scenarios are used is now clear. On the other hand, we will retain the scale of 0 to 600 m3 s-1, as simulated flows extend beyond 200 and 500 m3 s-1.

Thanks for the suggestion to add the coefficient of variation and the lag-1 autocorrelation. However, this would not allow us to assess the ability of the chains to reproduce the reference temporal variations from one year to another. A more informative numerical criterion for this would be a NSE criterion for deterministic simulations (from MAR) and a probabilistic efficiency criterion (e.g. CRPSS) for ensemble simulations (from SCAMP). For our purpose, however, a graphical comparison of reference and simulated time series is, in our opinion, much more effective in demonstrating the ability of the chains to reproduce reference interannual variations.

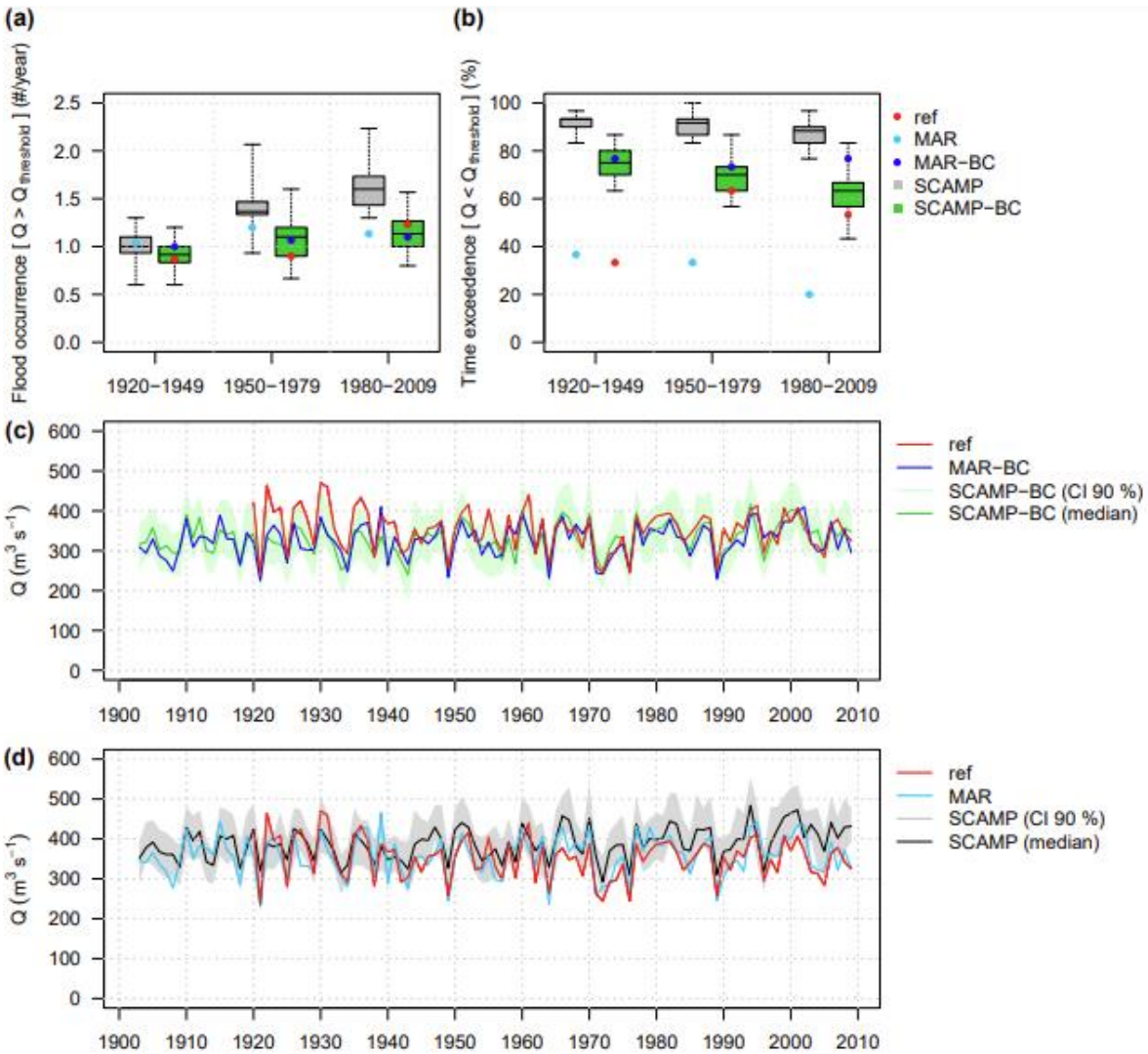



Figure 11. (a) Flood activity and (b) low flow activity at Rhône@Bognes for three 30-year sub-periods: 1920-1949, 1950-1979 and 1980-2009. (c) Mean annual discharges at Rhône@Bognes for the period 1902-2009 simulated with MAR-BC and SCAMP-BC. (d) The same for simulation with MAR and SCAMP. The grey and green bands represent the confidence interval at 90 % level. The median scenarios are indicated by the black and green solid lines. The "references" are observed discharges for the 1920-1960 period and simulated discharges from observed weather for the 1961-2009 period. MAR/MAR-BC: hydrological simulation forced by the raw/bias-corrected weather scenario produced with the dynamical downscaling model. SCAMP/SCAMP-BC: hydrological simulations forced by the raw/bias-corrected weather scenarios produced with the statistical downscaling model.

**RC3.15** Figure 11b: it is written "The results for low flow activity are less satisfactory, especially for the 1920-1949 sub-period (Fig. 11b). The number of low flow sequences below the threshold in both simulations is indeed twice that of the reference. This suggests a limitation of both downscaling models to simulate long persistent dry sequences." This interpretation is perhaps a bit too limited; most readers might not know that there was a e.g. a heat wave in 1946 and a period of strong melting in the 1940 due to enhanced solar radiation (Huss et al., 2009); there were also some very cold winters in this period and some very warm years (see here https://www.meteoswiss.admin.ch/climate/climate-change.html). I would give some more details here and perhaps use other subperiods to better understand what is going on with the modelling chain? This low flow underestimation is really too striking to not discuss it in far more depth. This is really important because many modelling chains do a rather poor job for snow-influenced low flow but this is rarely discussed. And: it is important to be precise here: is the reference for period 1920-1960 simulated or observed? To understand if the lake management can or cannot explain the low flow differences.

> **AC3.15** Thank you for this comment. We do not currently have a clear explanation for these results. We will do some more investigations and clarify this issue if possible, and if not possible, we will mention that this issue will need to be investigated in future works.
>
> Note that possible explanations have already been proposed in the conclusion: "To some extent, the simulations also reproduce low flow sequences and annual floods quite well. For the first half of the century, the agreement with reference discharges is lower (but still reasonable), likely due to lower data quality (ERA-20C and discharges data) and/or to some modeling assumptions and choices (e.g. signature-based calibration, stationarity assumption)."
>
> The "references" are observed discharges for the 1920-1960 period and simulated discharges from observed weather for the 1961-2009 period. See our answer to the next question.

**RC3.16** In fact, do I understand correctly that the reference is not always the same for all result plots (which would not be ideal and should be mentioned in all plots, in the caption)? At line 382 following it is written "Simulated year-to-year variations of mean annual discharges are next compared to the "reference" ones over the 1920-2009 period (Fig. 11c). The "references" are observed discharges for the 1920-1960 period and simulated discharges from observed weather for the 1961-2009 period."

> **AC3.16** We acknowledge that this is not an ideal configuration, but unfortunately we did not find a better way to carry out the evaluation. Ideally, the reference should be the observations. However, for the 1961-2009 period, observed discharges are significantly perturbed by dams (most of which were built in the 1950s). See the figure below from Hingray et al. (2010). Comparing simulated discharges from meteorological scenarios with observed discharges is therefore meaningless. The observation was thus replaced by a proxy: the discharges simulated from observed weather. The "references" are observed discharges for the 1920-1960 period and simulated discharges from observed weather for the 1961-2009 period for Fig. 11 only.
>
> This point will be specified in the caption of Fig. 11.

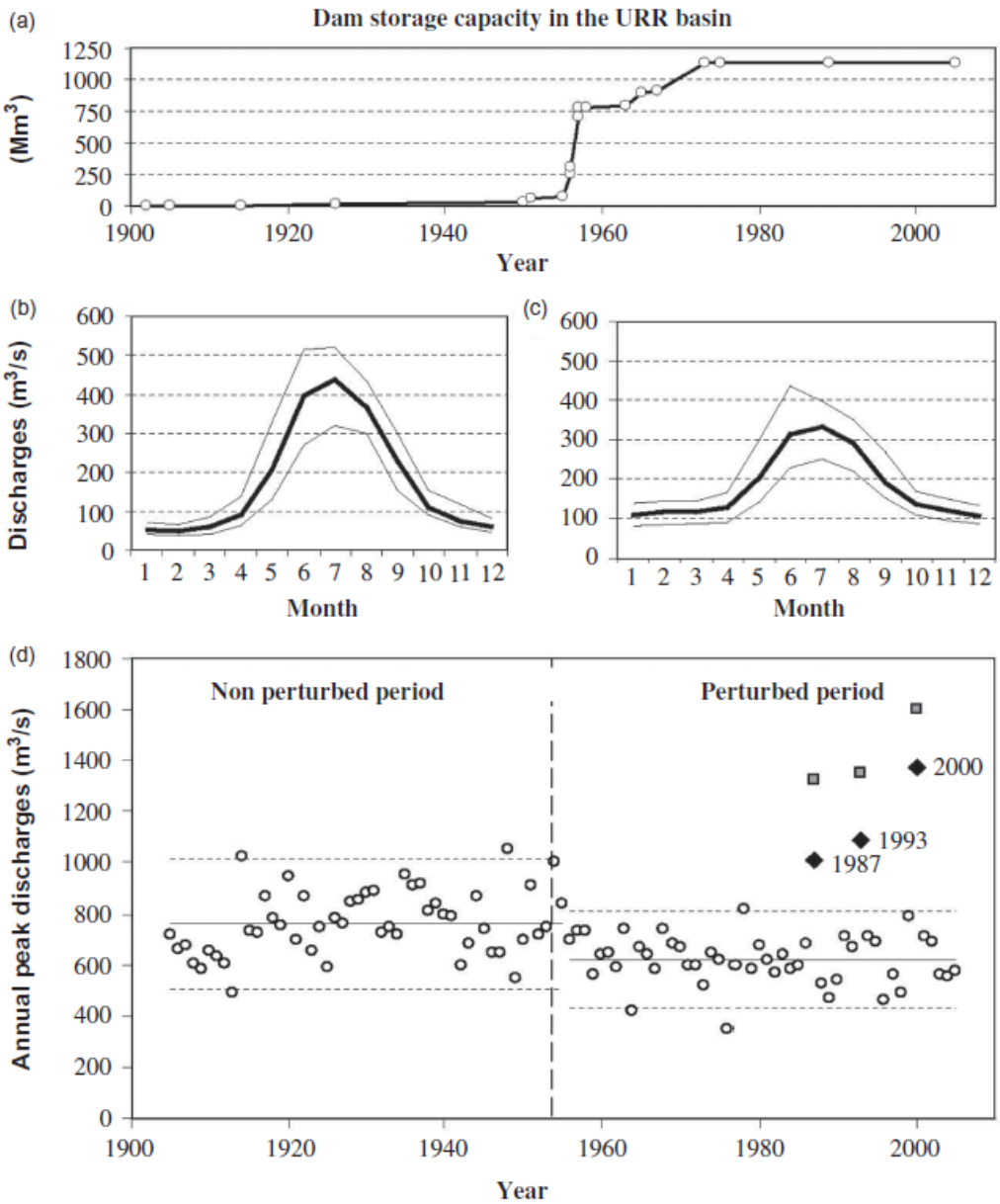



**Fig. 1** Effects of water works on the URR basin hydrology: (a) dam storage capacity evolution over the 1905–2005 period; (b) and (c) 10, 50 and 90% percentiles of the mean monthly discharge at Porte du Scex for 1905–1955 and 1955–2005, respectively; and (d) annual maximum peak discharge. For 1987, 1993 and 2000 floods: ◆: observed discharge; □: reconstructed discharge (from Hingray *et al*., 2009).

Discussion

**RC3.17** What could explain a warm bias in the lower atmospheric layers of the model? Besides: would be interesting to see winter and summer lapse rates separately to gain more insight ?

**AC3.17** The warm bias found in the MAR-ERA20C simulation was pointed out in Beaumet et al. (2021). This bias is found mainly during the winter, associated with a too strong lapse rate. It might be linked to both surface and atmospheric processes. Looking for the exact causes of this bias is a complex discussion, and we estimate that it is out of the scope of our article.

**RC3.18** Line 493 following: "Note that in our simulations, the dry bias in the precipitation input is probably corrected via an adjusted parameterization of the hydrological model, (..)". I fully agree with this explanation but I would make it clearer for non experts; now it reads like if someone adjusted it, perhaps say something like: "the dry bias is probably compensated during hydrological parameter optimization; parameter optimization generally results in forcing the model to close the water balance; conceptual reservoir-based models as the one used here can thereby compensate missing rainfall input by lowering the evapotranspiration losses. This problem is well known but very rarely discussed. An example is the work of Minville et al. (2014).

**AC3.18** Thank you for this comment. We will adapt the text to clarify this point.

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