

Interactive comment on “Multivariate stochastic bias corrections with optimal transport” by Yoann Robin et al.

Yoann Robin et al.

yoann.robin@lsce.ipsl.fr

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1 Synopsis

The manuscript studies bias-correction methods for climate models (that are here considered as dynamical systems). A new method based on optimal transport theory is suggested: given results from two models X and Y and considering these as probability distributions, a joint probability law is determined from optimal transport theory that couples the two distributions for X and Y in a certain optimal sense (i.e., least work in transforming X into Y). The ensuing joint distribution can then be used to obtain stochastic corrections for data samples, by sampling from the conditional distribution of Y , given X (or vice versa) - which is an interesting idea. The authors

C1

then continue to suggest corrections for the case of observing two models at two different points in time. Here they propose to estimate the optimal transport from Y^0 to X^0 and from X^0 to X^1 . Together, these two joint distributions are used to obtain values of Y^1 from Y^0 , by adding random realizations of the differences between X^0 and X^1 to Y^0 , scaled by the covariance matrix between X^0 and Y^0 . Simulations with a non-stationary, perturbed variant of the Lorenz model show the potential for reconstruction of the (known) distribution of Y^1 . Results comparing two different climate models are somewhat less satisfactory and show that time evolution in both models seems to differ in some important aspects that are not captured too well by the method then. However, the correction method at single time instances still works well.

2 Assessment

The manuscript is well written and the topic is suitable for HESS. The explanation of the proposed methods is well executed, being correct and relevant, while being concise and leaving out unnecessary mathematical details. All in all a manuscript that I enjoyed reading. It is actually quite thought-provoking, since the correction method proposed for the "non-stationary" case could also be done differently - maybe it would be worth to investigate these other options also? I can recommend the manuscript for publication, but I would like to invite the authors to comment on a few issues (see detailed comments below), mostly in order to further increase the relevance.

Thank you for this comment. This is right: the non-stationarity can be done differently but some preliminary tests performed on another approach gave disappointing results (see our answer to comment about Section 2.4). We propose this approach because we can define properly the dynamic of the model, and the idea "to transfer the dynamic

C2

to observations" is justified. The reverse hypothesis (transfer the bias) is more problematic, because we do not really understand "what" the bias is, and assuming the bias is the same at two time period is not justified for us. More investigation (in progress) is necessary to understand first how the dynamics of the model is preserved (or not) using more robust indicators coming from dynamical system theory (see, e.g., Freitas, (2010,2012), Lucarini (2012)), and secondly to define what the "bias" is.

- Freitas, A. C. M., Freitas, J. M. and Todd, M. (2010). "Hitting time statistics and extreme value theory". In : Probability Theory and Related Fields 147.3, p. 675–710. doi : 10.1007/s00440-009-0221-y.
- Freitas, A. C. M., Freitas, J. M. and Todd, M. (2012). "The extremal index, hitting time statistics and periodicity". In : Advances in Mathematics 231.5, p. 2626–2665. doi : <https://doi.org/10.1016/j.aim.2012.07.029>.
- Lucarini, V., Faranda, D. and Turchetti, G. and Vaienti, S. (2012). "Extreme value theory for singular measures". In : Chaos : An Interdisciplinary Journal of Non-linear Science 22.2, p. 023135. doi : 10.1063/1.4718935.

3 Detailed comments

3.1 Equation 1

The authors use the quadratic Wasserstein distance. This has a number of nice theoretical properties, but in statistics the L^1 Wasserstein distance (i.e. without the square, then known as the "Kantorovich-Rubinstein" distance) would be a more robust choice — although potentially losing uniqueness of the solution then — and might be considered. Please add a few words about the choice of the distance here.

C3

3.1.1 Response

We have considered the use of the L^1 Wasserstein distance. In the case of the Lorenz 84 attractor (Section 3 of manuscript), using the L^1 or the L^2 distance does not change the correction for OTC and dOTC method. But for the example with three bivariate Gaussian (dOTC correction, see Section 2.5), the L^1 distance gives results that are not satisfactory (see Fig. ??, same experiment as Section 2.5, with L^1 distance). In the y -axis, the standard deviation is equal to 0.61, whereas 0.125 is expected. So we have chosen the L^2 distance.

3.1.2 Modification

A short explanation has been added p. 6, l. 13-14.

3.2 Section 2.4

The way the "non-stationary" case is addressed is very interesting, but also somewhat controversial. Both the CDF-t method and the authors' work is based on assuming that time evolution is somehow the same for the two models/systems considered, i.e., that

$$\mathcal{T}_{Y^1, Y^0} = \mathcal{T}_{X^1, X^0}$$

This might be justified from a dynamical point of view, but from a statistical (or data science) point of view it seems more reasonable to actually consider that the bias between the two systems remains the same, i.e., that

C4

$$\mathcal{T}_{Y^1, X^1} = \mathcal{T}_{Y^0, X^0}$$

The transformation would then be the opposite, $G^1 = F^1 \circ (F^0)^{-1} \circ G^0$ instead of $G^1 = G^0 \circ (F^0)^{-1} \circ F^1$ as for the above. Has nobody considered this so far in the literature? Why not? The paper would benefit a lot from a (short) discussion of this second possibility! (and maybe even a few results with it)

3.2.1 Response

As far as we know, no one in the literature has considered the opposite relationship. Almost all bias correction methods assume that the “bias” is constant, but this notion is not clearly defined. For example, with our formalism, the Quantile Matching method assumes in practice the stationnarity (i.e. $\mathbb{P}_{Y^0} = \mathbb{P}_{Y^1}$), and CDF- t a transfer of the dynamic. At the beginning of our work, we wanted to use both hypotheses simultaneously (i.e. $\mathcal{T}_{Y^1, Y^0} = \mathcal{T}_{X^1, X^0}$ and $\mathcal{T}_{Y^1, X^1} = \mathcal{T}_{Y^0, X^0}$). In this case, the only solutions are trivial: $\mathbb{P}_{X^0} = \mathbb{P}_{X^1}$ or $\mathbb{P}_{X^0} = \mathbb{P}_{Y^0}$ or $\mathbb{P}_{X^0} = \mathbb{P}_{Y^1}$. We therefore concluded that it was necessary to choose one of the two hypotheses but it is impossible to have both in practice. We retained the hypothesis of the evolution that made it possible to reconstruct the Lorenz84.

3.2.2 Modification

We change the paragraph:

“Our definition of non-stationary bias correction assumes a transfer of the evolution of the model to observational world. But the evolution of observation can be different, and the resulting correction can be also different from observations. This methodology is

C5

justified because we want to keep the evolution of the model, even if the dynamic of the model is different of the dynamic of the observations.”

For (p. 8, l. 11-17 and p.9, l. 1-2):

“Note also that the reverse hypothesis $\mathcal{T}_{Y^1, X^1} = \mathcal{T}_{Y^0, X^0}$ could be considered, meaning that the bias is learned, and transferred along the dynamic. In this case, the correction of example given in Section 3 does not correspond to the reference (not shown), so we rejected this assumption. Thus, our definition of non-stationary bias correction assumes a transfer of the evolution of the model to observational world. Indeed, climate change is one of the main signal that we want to account for in the projected corrections. However, the change in the observations can be different, and therefore the resulting corrections can be also different from observations. Nevertheless, this methodology is justified because different simulations can have different variations, e.g., the four RCP scenarios provide four different simulations, giving four different corrections. This is also true for different climate models, which can show different changes. This information is therefore kept in the corrections.”

3.3 page 7, line 18

Related to the previous item, “because we want to keep the evolution of the model” is a somewhat unscientific statement. Why do you want to assume that the time evolution is the same - what are the reasons that make this a suitable assumption here, especially for complex climate models?

3.3.1 Response

The time evolution of the model represents a potential (biased) future of distributions of observations, constrained by the hypothesis over some natural and/or anthropogenic

C6

forcing, and the physics of the model. We assume that the goal of bias correction is not to change these hypotheses and the physics, but to keep it (otherwise, why use a model?). According to our answer to the previous question, we cannot keep simultaneously the bias evolution and the time evolution. Thus, we kept the time evolution hypothesis.

3.3.2 Modification

See previous question/answer.

3.4 page 8, line 16

" whereas it increases between ..." - It does, but only here for this example, not in general. Please mention this, to avoid confusion.

3.4.1 Response

We agree.

3.4.2 Modification

The sentence "...whereas it increases between Y^0 and Y^1 ." has been corrected as follow: "...whereas it increases between Y^0 and Y^1 in our example."

C7

3.5 page 8, step 3

The proposed adaption seems somewhat unnatural to me. Looking at Figure 3, I would think that Figure 3a is a more appropriate reconstruction than Figure 3b, since it captures the important fact that the uncertainty about the values increases when transferring the assume dynamical evolution. This seems actually desirable! But of course it all depends on the goal here. Please comment!

3.5.1 Response

Your suggestion (the increase of uncertainty) is very interesting, and offers a point of view that we had not considered. The reason we introduce this correction factor is that the transferred vectors do not form a reasonable transport plan because they can invert the quantiles. High temperatures can be transferred to low temperatures, even if the use of OTC after for the correction between X^1 and Y^1 could mask the problem.

3.5.2 Modification

The problem of quantile inversion has been added (p. 10, l. 2-3): *Furthermore, here the quantiles are inverted (low values are moved to high values)*

3.6 Conclusion

Finally, it could be nice to discuss in the conclusions the relationship with copulas - which are functions that capture the dependence structure between two random variables X , Y , as does the transport plan here so there is an underlying " optimal transport copula". Mentioning this connection could maybe make the work presented here interesting for a larger readership.

C8

3.6.1 Response

The link between copulas and optimal transport has been considered, but not treated.

3.6.2 Modification

A few words have been added in the perspective.

4 Minor comments

4.1 page 3, line 13

Just a comment: the nomenclature is a bit strange (this seems to have historic roots in this field, so is not the authors' fault), the name "transfer function" does not seem a good choice as it means something quite different in dynamics. It would be more appropriate to simply call this a "map".

4.1.1 Response

"transfer function" means two different things in dynamical system theory and bias correction context. The term "transfer function" being associated to the map correcting the bias in bias correction, we prefer to keep this term.

C9

4.1.2 Modification

To avoid confusion, we have been changed the sentence "...is a transfer function $\mathcal{T} : \mathbb{R}^d \rightarrow \mathbb{R}^d \dots$ " for "...is a map $\mathcal{T} : \mathbb{R}^d \rightarrow \mathbb{R}^d$, called a transfer function,...".

4.2 page 3 , line 15

maybe add "deterministic" before "transfer function" ?

4.2.1 Response

Agreed.

4.2.2 Modification

The change has been done.

4.3 pager 12, line 19

"close" instead of "closed"

4.3.1 Response

Indeed.

C10

4.3.2 Modification

The change has been done.

4.4 page 8, line 22-23

A matrix is not "definite", so you probably mean "positive-definite" here in both cases?

4.4.1 Response

Indeed.

4.4.2 Modification

The change has been done (p. 10, l. 11-13).

4.5 page 14, line 25

"significant" instead of "significative"

4.5.1 Response

Indeed.

4.5.2 Modification

The change has been done.

C11

4.6 page 14

The discussion of the results shown in Figure 5 is quite dense, it could benefit from a few more words?

4.6.1 Response

The discussion has been detailed.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2018-281>, 2018.

C12

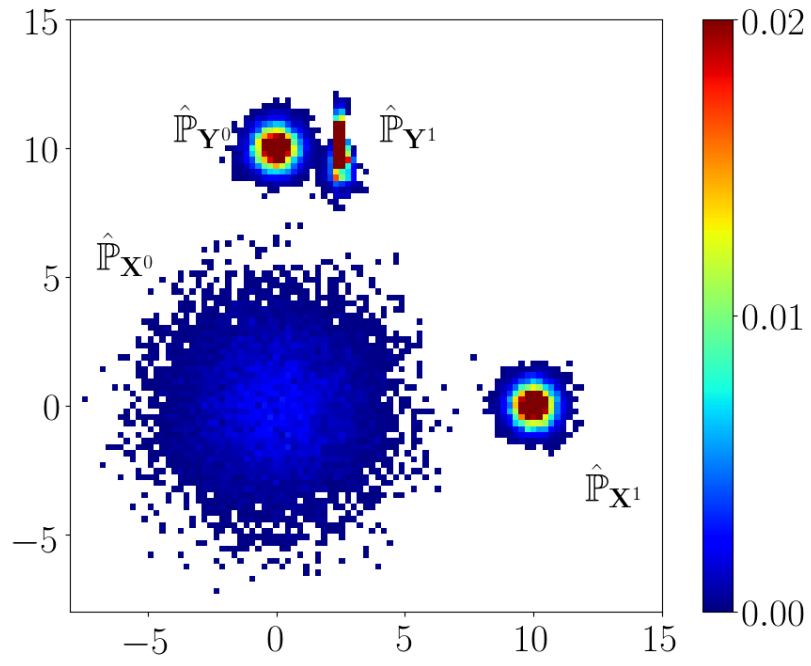


Fig. 1.