

EGUsphere-2022-1385 response to reviewer 2

<https://egusphere.copernicus.org/preprints/2023/egusphere-2022-1385/>

Major comments

The presented EOF technique does not pursue the same target as the original common EOFs introduced by Flury (1984) and recently applied by Hannachi et al. (2022). These common EOFs find a set of basis vectors that approximately maximizes the variance within a number of datasets (each from one projection) simultaneously. The EOFs proposed by Benestad et al. find the exact EOFs that maximize the mixture of within variances (within each projection) and between cross-covariances (cross-covariance between all pairs of projections) of the combined dataset. As these are two different kinds of optimization, I would suggest to **find a new name for the presented technique**.

Besides, it would surely be very helpful to discuss the implications of this difference on the meaning of the resulting EOFs. The technique could further be **contrasted to multivariate EOFs, where all monthly values of any one projection are stacked in space-direction instead of time-direction**, as was done by Sanderson et al. (2015) to represent the similarity between projections in a joint multidimensional space, and/or to one of the many **tensor decompositions, where the space-time matrices of the individual projections are stacked along a third (model) direction**, and components are found, which present the main variations along those three directions (e.g. Cichocki et al., 2015). Aside from that, there exists a vast and slightly chaotic literature on multigroup/ multiblock/multitable PCA methods, which is also concerned with contriving ways to combine datasets of differing origin in one joint PCA.

The discussion of the presented applications seems, at least in my opinion, very much centered on the “easy” cases, where the variance is represented overwhelmingly by the first EOF and the reference lies well in the middle of the ensemble (seasonal cycles, interannual variability of TAS and PSL, and TAS trend). The authors could take the more complicated situation in the interannual variation of PR as an opportunity to **elaborate further on the inclusion of more than one PC in the subsequent evaluation, and on the possibility and consequences of the reference not falling inside the ensemble**.

I further suggest to consider a variation of the proposed method: to include only the ensemble of projections in the EOF analysis, and to project the reference, which might also be more than one references, onto the EOFs afterwards. It would appear to me as a “cleaner” approach not to meddle projections and references. On the other hand, in such

vast ensembles as used here, the exclusion of the reanalysis will hardly make any appreciable difference.

Sanderson, B.M., Knutti, R., Caldwell, P. (2015): A representative democracy to reduce interdependency in a multimodel ensemble. *Journal of Climate*, 28(13), pp. 5171-5194

Cichocki, A., Mandic, D., De Lathauwer, L., ... (2015): Tensor decompositions for signal processing applications: From two-way to multiway component analysis. *IEEE Signal Processing Magazine*, 32(2),7038247, pp. 145-163

Thank you for your comments. The point about using a different name is well-taken, and the term ‘common EOF’ in our opinion is not optimal either, but was used by Barnett (1999)¹ who applied an ordinary EOF analysis to a joint dataset, just as we did here. Also, we have used this approach since 2001 as a framework for representing predictors in empirical-statistical downscaling, albeit for reanalysis-GCM pairs where the reanalysis has been used for calibration and GCM for projection. Nevertheless, this point is well-taken and will be included in the revised paper, as will the contribution to the body of knowledge represented by the two papers. Thanks for these references.

The suggested variation of our analysis is interesting, and we will discuss it in the revised paper. We nevertheless want to stick to our original approach, which also was used in Barnett (1999), as the motivation behind this study is the use of the common EOFs as predictors in empirical-statistical downscaling where they also have a role in the evaluation of both GCMs and the downscaled results. Furthermore, our hypothesis is that the subselection of ERA5 and the GCM data represent the same variable, region and time period, and hence the same statistics.

The point concerning a more complicated situation in the interannual variation is also an important one. The revised paper will discuss further the option of including more than one PC in the evaluation. And there is always a possibility that the reference does not fall inside the ensemble spread for a given PC, which would indicate that they belong to different statistical populations. This matters most for PCs representing a large fraction of variance, but can be ignored for high-order PCs associated with negligible variance that represent “numeric noise”.

Minor comments

Line 235 ff.: Is there any evidence that supports the proposition of independence between ensemble members? The QQ-plot suggests Gaussian distribution, but to my knowledge the QQ-plot gives no hint concerning independence. Furthermore, this would contradict the widely-recognized notion of strong interdependence between climate models.

¹ <https://shorturl.at/dqxAG>

From a physical point of view, we know that these models reproduce chaotic and stochastic variability on decadal scales and this is especially apparent if the ensemble is made up of simulations with one common model (Deser et al., 2012). For multi-model ensembles, there is also an additional component: model differences.

We should indeed expect a strong interdependence between climate models since they are built to represent the same physical system - what we also desire is that the aspects that are not well-established and uncertain should involve different choices/methods so that they also provide a decent sample of the parameter space of unknowns. But in practice, different groups often copy others' attempts so that model uncertainties are not so well sampled. Nevertheless, the simulated stochastic/chaotic regional internal variability appears to be more pronounced so that these concerns are secondary in this case.

PC plots in all figures: I find it difficult to distinguish the black curve (reanalysis) against the background of the dark blue curves (CMIP6).

Thanks for pointing this out - the clarity of the figure will be improved in the revised paper.

References:

Deser, C., Knutti, R., Solomon, S., and Phillips, A. S.: Communication of the role of natural variability in future North American climate, *Nature Climate Change*, 2, 775–779, 2012, DOI: 10.1038/NCLIMATE1562