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Hydrology and Earth System Sciences Discussions

Interactive comment on "Regional climate models downscaling in the Alpine area with Multimodel SuperEnsemble" *by* D. Cane et al.

Anonymous Referee #1

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D. Cane et al aim at providing one future scenario of temperature and precipitations over two alpine domains with different resolutions, which would be well suited for impact studies in hydrology and ecology among other aims. In particular, their goal is to (1) remove the severe biases of climate models in a control period and (2) weight the simulations of different RCMs to give more confidence to the better models over the control period and finally provide a single future scenario.

This issue has been raised more and more for several years to look for local adaptation strategies to climate change. Therefore, I acknowledge that the authors try to address a crucial problem resulting from the large discrepancies between the expectations of policymakers and local stakeholders and the very complex outputs from the climate

modelers community (numerous models with significant intrinsic errors). For this, the authors have chosen to apply two statistical methods generally used in the context of meteorological forecasting to post-process RCMs outputs from the European ENSEMBLES project. One method is applied on temperature fields, the other one for precipitation.

Unfortunately, I am afraid that the chosen method is absolutely not relevant in the context of climate modeling. However, I might have misunderstood some points because the methodology is not described in a sufficiently complete and accurate way.

If I have well understood, in the context of meteorological forecast:

- For temperature, the optimal forecast is a linear combination of the anomalies between the forecast and climatology of each of them.

- For precipitation, the PDF of observed precipitation is established for each possible value forecast by a given model. A multi-model PDF is then computed giving different weights on each model-specific PDF. I understand that this multi-model PDF is specific for a given precipitation forecast, but it is unclear in the paper. Is this precipitation forecast an average of the model ensembles for a given day? The weights are computed as a function of the CRPS score of each model-specific PDF. Is the CRPS computed at a daily time step?

Both methods are probably very useful in operational forecasting to account for different models and to give a higher confidence to the statistically best ones. Their application on RCMs driven by ERA40 reanalyses may also be relevant, as we can expect the RCM to reproduce chronological variations of the meteorological conditions in this context.

However, it is unclear in the paper whether the coefficients of the linear combination for temperature and the weights of the PDF for precipitation are recalibrated when the RCMs are driven by GCMs. If not, the method would not account for the biases induced by GCMs. It would not make any sense to apply the weights estimated with the ERA40 forcing whereas huge differences can be due to the GCMs. If the coefficients are recalibrated, I wonder how the weights are computed. Indeed, without any additional information, I guess the weights are derived from a linear regression for temperature and from CRPS score for precipitation, both on a daily basis. If this is the case, I'm afraid that such an application of the statistical method would make no sense in this context. Indeed, the GCM forcing (usually greenhouse gases, sometimes solar and volcanic forcings) do not allow the GCM to reproduce realistic chronological variations of meteorological variables. A GCM simulation cannot be treated in a similar way to the forecast of a meteorological model. A GCM is meant to reproduce some climatological statistical properties over a given reference period of 20 or 30 years (e.g. Glecker et al, 2008 ; Sheridan and Lee, 2010). Therefore looking for an optimal combination of models to reproduce daily temperature or precipitation observations is definitely not relevant.

In both cases, I think that these statistical methods are probably very useful in operational forecasting, but not relevant in climate modeling. Therefore, I don't recommend the publication of this paper in HESS and I suggest the authors to investigate other unbiasing methods currently used in such applications (cf. review of Maraun et al, 2010). For instance, percentile-percentile corrections methods (Dequé, 2007) or other methods based on statistical properties and not on chronological series (e.g. Vrac et al, 2007) are recommended to remove the biases of GCM and RCM.

Furthermore, if the weighting of climate models is expected by the "impacts community", I think in a scientific point of view that we should never present a single future projection by weighting models without an associated uncertainty range. The word "uncertainty" does not appear in the manuscript. Numerous studies have presented the various sources of uncertainties in climate modeling and in hydrological applications (e.g. Chen et al, 2011 ; Grillakis et al, 2011) and should be quoted and used. Assuming that the differences between GCM and recent climate observations (even in

climatological properties) are sufficient to estimate weights for the future is not really justified. Indeed, these differences are not only due to the physical bases of climate models, they are also impacted by the internal variability in the GCM with a decadal or pluridecadal time scale, representing the intrinsic chaotic variability of climate (e.g. Lucas-Picher et al, 2008). There is no evidence that the models which are the closest to observations in the past are the most reliable in the future (Weigel et al, 2010).

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