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Interactive comment on "A Bayesian posterior predictive framework for weighting ensemble regional climate models" *by* Yanan Fan et al.

Anonymous Referee #1

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The authors report on a statistical method loosely connected to bayesian analysis to postprocess ensemble simulations of tegional climate models. they apply the method and report results for seasonal temperatures in australia over two 20 year periods. The authors claim that their approach is entirely novel, which I do not see but which is a matter of definition of "new".

More important i found a significant lack of theoretical background, rather the model is set up in a rather adhoc fashion: we need a weighted combination of regional climate models -> so why not taking bayesian model averaging with some weights; where to get the weights from -> why not taking the likelihood with some "uninformed prior"; how to calculate the weights -> why not taking mcmc; where to get the ensemble from -> why not taking the regional climate simulations.

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Discussion paper

The content of the paper is certainly worth to be published but not in this very way as it is presented currently. Papers in GMD should not only report on the technical aspects but also on the theoretical background because this allows to draw conclusions about the assumptions made for the specific implementations. Furthermore even the technical aspect is only mildely covered because at no point except in the very last sentence it is said that the current implementation relies on (univariate! not mentioned!) normal distributed random variables. Additionally the use of a Bayesian approach is only marginally. Firstly, the modelling of uncertainty is rather adhoc (see my remark above) in the sense that the model parameters especially the precision values of the residuals are treated in a non common fashion, standard approaches eg in described in Gilks et al 1996 at least consider the normal-invers gamma model with a wide prior on the hyperparameter of the invers gamma component , secondly the treatment of observations in the likelihood and the treatment of simulations in the prior does not consider the dependency between the residual components. This holds for the actual analysis where the epsilons could be modeled as an AR process in time giving rise to a multivariate normal for the x_m which is then transformed by a linear projection into a bivariate normal for the trend and offset. The likelihood can be written as a function of the same projection jointly upon observations and simulations such that the negative loglikelihood looks like this

$$nll \sim (\theta_m - \theta_o)^t P^t (\Sigma_m + \Sigma_o)^{(-1)} P(\theta_m - \theta_o) + log(det(\Sigma_m + \Sigma_o))$$

where$\theta_{(m,o)}$ are bivariate vectors containing the offset and slope of the linear fitted function (or any amplitude of a generalized additive model) and P is a matrix containing in its columns the 1's for the offset and the $(t - t_0)$ for the slope (or any function g_k in case of the generalized additive models). note that in this approach the uncertainty in the covariance matrices $\Sigma_{(m,o)}$ is not yet treated, but this is possible if eg $\Sigma_o = \sigma_o^2 I$ (I identity matrix) and setting an invers gamma prior for σ_o^2.

The inclusion of the P matrix shows that the currently chosen way of trend fitting introduces a correlation between the error in offset and trend because the function $(t - t_0)$ does not sum to zero over the full time interval and therefore any error in the slope will produce an error in the average which has to be compensated by a negative deviation in the error of the mean.

This shows that the analytical analysis of the approach using normal distributions firstly does not need a mcmc numerical solution and allows to draw important conclusions about the characteristics of the method.

Similar remarks can be made for the predictive probability densities, also here a lot can be learned from the (multivariate) normal densities. Note that the multivariate densities always include the univariate case but not the otherway around.

Summary The paper is worth to be published in GMD but the authors should comment/add modifications according to my general remarks. Detailed comments are found in the annotated pdf document.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/gmd-2016-291/gmd-2016-291-RC1-supplement.pdf

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