Report on the revision of the paper
A Bayesian posterior predictive framework for weighting ensemble regional climate models
by Fan, Olson and Evans, submitted to Geoscientific Model Development

I am not entirely happy with the way the authors have addressed the comments by the reviewers.

I think the authors could do a better job in presenting and justifying their method. I did not claim that Buser et al. (2009) use Bayesian model averaging, my argument was simply that the methods of both Buser et al. (2009) and of this paper fit into a general framework of Bayesian analysis in a situation where for some parameters the available data provide no information. In such a situation, an informative prior has to be used. I think that this would help the readers to better understand the method and it would also provide a good answer to the criticism of reviewer 1 who finds the method very ad-hoc. In particular, I find it misleading that the authors write that they use non-informative priors throughout because the informative part of their prior is hidden.

Let me briefly repeat this general framework in the notation of this paper. The parameters for which we have no information from data are the intercept a^f, slope b^f and standard deviation σ^f of the future climate y^f. The informative prior used by the authors says that there is a perfect model that has the same parameters as the observations both in the current and the future period (so for instance $a_p = a_m$ and $a^f = a_m^f$ for one model m). The prior does not specify which model is the perfect one; the observations and model outputs in the current period are used to estimate the probability that each model is the perfect one. This is a valid and interesting solution. In Buser et al. (2009) a different approach was chosen where for instance the intercepts of model m are decomposed as $a_m = a_p + \beta_m$ and $a_m^f = a^f + \beta_m + \delta\beta_m$. An informative prior is then used for the bias changes $\delta\beta_m$ because the data give information only about a_p, β_m and and the sum $a^f + \delta\beta_m$.

The addition on l. 19 of p. 3 "In practice σ_p has additional terms" does not reflect my concern that in practice the standard deviation σ_m of many models is **larger** than the standard deviation σ_p of the observations. A different modeling approach would be needed to take this into account.

I stronlgy disagree with the additional text on l. 8-12 of p. 2: The paper bys Buser et al. (2009, 2010) and by Kerkhoff et al. (2014, 2015) also include

internal variability in a principled way, and presumably other authors have done this too. The observation that the projected change of a model is positively correlated with present-day internal variability is due to Buser et al. (2009) (termed "constant relation"). And the method of this paper does not take this correlation into account.