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Interactive comment on "The ability of a GCM-forced hydrological model to reproduce global discharge variability" *by* F. C. Sperna Weiland et al.

Anonymous Referee #2

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This study presents the simulation results of the global hydrological model PCR-GLOWB in terms of river discharge statistics for 19 large river basins worldwide. The model is driven by bias-corrected daily climate data of twelve GCMs and the ERA-40 reanalysis data. Bias correction is performed for precipitation, temperature and potential evaporation relative to monthly mean data of the CRU TS2.1 data set. The paper has its strongest and most novel part in demonstrating that the GCM-driven models do not adequately represent inter-annual discharge variability and annual autocorrelation. However, major parts of the paper suffer from insufficiently explained methods, unjustified results and conclusions, and the questionable approach of setting the ERA-40 driven model as the reference model. In particular, I suggest that the following aspects should be thoroughly addressed in a revised version of the paper:

1) The reference for all model runs should be observed river discharge. It is not sufficiently justified why to use the ERA-40 based model as a reference, given the limitations of this reanalysis data set for hydrological purposes (e.g., Hagemann et al. 2005, Validation of the hydrological cycle of ERA 40). ERA-40 may result to be superior to the GCM-based models, but if so this should be an independent result of this study instead of setting it a priori.

2) With the approach for bias correction applied in this study (i.e., correcting to mean monthly values of the CRU data set), differences in the discharge simulation results of the PCR-GLOWB model can mainly be expected to be due to (a) the different sub-monthly climate variability of the different GCM data sets, and (b) differences in the inter-annual variability in the GCM data. From this perspective, it is not clear at all why single GCM-based model runs show up with completely different mean monthly river discharge regimes for some river basins (Fig. 2), i.e., 'deviating regime curves' (page 701, line 21), although they are driven with input data with similar monthly climate regimes. Thus, the conclusion that 'few deviating GCMs can bias the discharge statistics ...' (page 705, line 18) is not well founded or at least not sufficiently explained. Also Chapter 3.2.2 on the differences in (monthly) timing of discharge peaks is unreasonable in view of similar monthly climate forcing.

3) Derivation of potential evaporation, Chapter 2.2.1, page 694. It is not clear how potential evaporation for this study could be calculated based on CRU TS2.1 data and using the Penman-Monteith equation, given that the CRU data do not include wind speed and net radiation as required for Penman-Monteith.

4) Information on PCR-GLOWB model performance relative to observed river discharge is shortly given in an appendix only. This part is insufficient and poorly written. From a general perspective, the approach of this study avoiding to 'focus on the correct reproduction of mean discharge or river regimes' (page 689, line 20) is questionable if

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the authors want to study in a sound way 'the influence of deviating GCM variability on the resulting hydrological variability' (page 689, line 26). I doubt whether this can be achieved with a model that does not properly represent the mean hydrological behavior of the studied river basins. More specifically,

4.1) using a logarithmic scale for comparing simulated and observed river discharge (Figure 9) is not adequate to evaluate model performance as errors may likely be several tens of percent without proper graphical representation.

4.2) using the sum of observed river discharge and water demand as a proxy for 'natural flow' as computed by the model is a misconception because actual water use (withdrawal water use) can be expected to be considerably lower than water demand.

4.3) The description of Figure 9 is imprecise, e.g., what is CRU_ERA; where are the deviations mentioned in the figure caption; the figure does not contain regimes contrary to what is mentioned in the text (page 707, line 7).

5) The conclusion that 'after bias correction the spread between regimes calculated with the 12 corrected GCM data sets is decreased' (page 705, line 10) is not justified as not results of simulation runs without bias correction are shown in the study.

6) The results of this paper are discussed and concluded without any reference to previous studies in this field (Chapters 3 and 4).

7) The discussion of lag-1 inter-annual autocorrelations in river discharge may gain from comparing with autocorrelation of basin-average precipitation time series. This may result in more concrete clues on whether (missing) autocorrelation is due to climate forcing, model errors, or the hypothesis of reservoir storage impact (page 704, line 20).

8) How has the CRU CLIM 1.0 climatology (page 697, line 7) been used within the approach taken in this study?

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