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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Seasonal drought prediction for semiarid northeast Brazil: About the added value of a process-based hydrological model" *by* Tobias Pilz et al.

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Dear referee,

thank you for your detailed and fruitful comments on our manuscript. All your critics shall be fully addressed in our Author's Response along with the revised manuscript. In this response we merely want to further discuss two specific points raised in your review.

> 1. A common sense about statistical models is that they may have good performance in the training period, but degrade in the validation period. This

> information is missing for all results (i.e., Figures 3-12). For instance, what is the time period for the simulation in Figure 3? How about separating calibration and verification periods? This separation is also critical for hindcast evaluation (Figure 4). In general, we have to use cross validation if there are not enough samples. And totally separating calibration and verification periods would be better. This will allow a fair comparison for statistical and dynamical models. In other words, one will never use any information in the validation period for the training or calibration. This is a basic rule for hydrological forecasting, but such important information is missing both in the text and figure captions. Both the calibration and validation results for both statistical and dynamical models should be presented. And the hindcast and forecast period should also be separated before comparison.

We are aware of the common procedure of calibration and validation using independent time periods in typical applications of hydrological simulation. However, we do not see a conflict to the procedure in our study, which included:

- **calibration** of parameters within time period 2003 to 2010 **forced by observations**
- model state initialisation 1980 to 2014 forced by observations
- assimilation of observed reservoir levels at the beginning of each year (i.e. **correction of model state**)
- **hindcasts** 1981 to 2014 (Jan to Jun) **forced by GCM predictions**

Hence, calibration and hindcast period, even though the time periods overlap, differ in a) the forcing, and b) model's reservoir storage which was updated by measurements.

Consequently, we think the hindcast and calibration runs can be regarded as independent. However, we admit the lengthy Methods section and the many Figures to be confusing at some points and shall seek for more clarification on that issue during the manuscript revision.

The other point to discuss is:

> 2. Regarding dynamical hindcast/forecast, a critical issue is whether the uncertainty in meteorological forecast affects the hydrological forecast greatly. Only one GCM (i.e., ECHAM4.6) is used in this study, which is not enough. In fact, the North American Multi-Model Ensemble (NMME) hindcast and realtime forecast data for precipitation and temperature are available for the public. There are a few global validation studies that cover the Brazil. Using multiple climate forecast models may provide an opportunity to quantify the uncertainty in the meteorological forecast. In other words, the conclusion that the statistical model outperforms the dynamical model may not be solely caused by the deficiency in hydrological model, and both the hydrological and meteorological parts should be addressed in the analysis.

We absolutely agree that precipitation forcing provided by the GCM certainly is a great source of uncertainty and interesting for further analyses. However, we discussed the use of further GCMs already during the conception of our study but finally decided against it because:

- we wanted to focus on the comparison of using two different types of hydrological models for drought forecasting
- we considered an ensemble of 20 realisations of ECHAM4.6, reflecting uncertainty arising from a specific GCM

- forcing uncertainty was explicitly addressed and discussed and in the paper, albeit in a simplified manner, see subsections 6.2 (p. 21 lines 12 to 23) and 6.4 (p. 24 lines 4 to 20)
- the paper is already rather long and extending it by a further aspect would make it even more complicated to understand

Therefore, we think properly addressing forcing uncertainty for drought forecasting would rather fill another full paper and should be investigated more comprehensively in an independent study. We suggest not to extend the current scope of the paper.

Kind regards,

Tobias Pilz (on behalf of all co-authors)

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