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Hydrology and Earth System Sciences Discussions

Interactive comment on "Uncertainty in climate change impacts on water resources in the Rio Grande Basin, Brazil" *by* M. T. Nóbrega et al.

M. T. Nóbrega et al.

collischonn@iph.ufrgs.br

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Interactive comment on "Uncertainty in climate change impacts on water resources in the Rio Grande Basin, Brazil"

We thank the anonymous reviewer (#2) for the careful reading and the helpful comments. In the next paragraphs we provide a point by point response to the queries.

Comment #1 In line 16 of page 6104 the authors mention that the 1981-2001 period was used for model validation but the results shown in figure 2 along with the comments in lines 22-29 of page 6105 refer to the calibration period (1970-1980), which is well-known to produce good skills since the parameters are estimated using this data.

I suggest the authors: a) Show some observed versus predicted values for the validation period (1981-2001). Maybe for at least one reservoir as they did for the Agua Vermelha reservoir; b) Show the skill metrics (volume bias, NS Nash-Sutcliffe coefficient and NSlog) for all reservoirs used for calibration and present them for both periods (calibration andvalidation) separately. If the number of reservoirs used is large, then maybe a boxplot would be good enough to show the average skill and the associated variability.

Answer #1 We agree with the reviewer that model performance during calibration period is generally better than during validation. We included a table (table 1 in the revised version of the paper) showing results statistics at ten different power plants were data was available, both during the calibration (1970-1980) and validation (1981-2001) periods. The statistics Nash-Sutcliffe and Nash-Sutcliffe of the logarithms of the streamflow show little change between the two periods. The values of volume bias, however, are generally higher during the validation period, but are still relatively low. Given that hydrological model performance is relatively well maintained from the calibration to validation period, we preserved figure 2 with data from the calibration period, and added information from validation period in the new table 1. It is worth to highlight that the statistics shown in table 1 are based on daily discharge data, while the hydrographs shown in figure 2 are of monthly values.

Comment #2 This paper requires a significant effort to be understood if you are not well familiar with specific climate changes concepts and definitions. Section 4 (climate projections) is explained in only one paragraph and some procedures/methods need clarification, for instance: a) Line 6, item (2); b) Lines 12-13: Baseline data. What is it and what is the purpose of it?; c) Lines 13-16: Why was the trend in the data removed? What variables were used? What method was used to remove the trend? Is there actually any trend in the data? If yes, does this trend relates to any possible trend in the streamflow data?

Answer #2 We agree that some parts of the paper are somewhat summarized, and do

HESSD

7, C4607–C4610, 2011

Interactive Comment

not give a full clarification of the methods, but these issues are more deeply discussed in other papers of the same special issue. We believe that the reader interested in getting more detail about the methods related to climate change scenarios can be satisfied by reading the papers by Todd et al. (this issue) and Gosling et al. (this issue). Baseline data is the data used to produce a control run of the model, giving the discharge values which are used as a basis of comparison. Trend was removed from the baseline data because we are mapping change relative to baseline so we would not wish to impose the 1961-1990 trend on all future scenarios. This modification introduced only slight changes to the original values. A linear de-trend was applied by extracting the residuals from a regression of the climate variables time series and an arbitrary time variable. We cannot conclude that the trend is related to increase in mean global temperature, and we have not investigated if the possible trends in meteorological data are related to trends in streamflow data.

âĂČ Comment #3 Lines 19-21, page 6101. The sentence is a bit confused and should be rewritten.

Answer #3 We changed the text. The new sentence is: For water resources management, principally for hydro-electric power (HEP) generation relying on sustained river flow, a more important result is the suggestion that low flows (those exceeded 90% of the time) would decrease by 58%.

Comment #4 Acronyms should be defined in the Abstract and along the text the first time they appear. For instance: SRES (Abstract, line 5), GCM (Abstract, line 7), GCMs (page 6101, line 10), TRMM (page 6104, line 1), etc.

Answer #4 We changed the text according to the suggestions.

Comment #5 I suggest the authors to switch the words when they state the river names (e.g. replace River Parana, River Paraguay, River Uruguay by Parana River, Paraguay River, Uruguay River and so on).

HESSD

7, C4607–C4610, 2011

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Answer #5 We changed the text according to the suggestions, however we believe that both forms (River Paraná or Paraná River) are valid.

Comment #6 T here are a few typos in the text that the authors should address. For instance: a)Abstract, line 2: replace "of the Rio" for "of Rio"; b) Line 17, page 6107: replace "all the durations" by "all durations".

Answer #6 We changed the text according to the suggestions.

References

Gosling, S. N.; Taylor, R. G.; Arnell, N. W.; Todd, M. C. 2010 A comparative analysis of projected impacts of climate change on river runoff from global and catchment-scale hydrological models. Hydrol. Earth Syst. Sci. Discuss., 7, 7191-7229, 2010.

Todd, M. C.; Taylor, R. G.; Osborne, T.; Kingston, D.; Arnell, N. W.; Gosling, S. N. 2010 Quantifying the impact of climate change on water resources at the basin scale on five continents – a unified approach Hydrol. Earth Syst. Sci. Discuss., 7, 7485-7519.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 6099, 2010.