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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.

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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 (#1) for the careful reading and the helpful comments. In the next paragraphs we provide a point by point response to the queries.

Comment #1 The results presented show that future river flow projections vary depending on the model used to generate the future climate scenario. However, the paper misses to show a clear concept of uncertainty and how it can be applied to the found results. Such a concept has been large explored under the idea of multi-

model ensembles and its advantages over using one only model has been already proved. Some papers that explored the uncertainties of future river flow projections related to different future climate scenarios are Krahe et al. (2009, HYDROLOGIE UND WASSERBEWIRTSCHAFTUNG, vol 53(5), 316-331) by creating projections of River Rhine discharge from different climate projections applied to the HBV-SMHI hydrological model; Christensen and Lettenmaier (2007, HESS vol 11, 1417-1434) for the Colorado River; and Nohara et al (2006, Journal of Hydrometeorology, vol 7(5), 1076-1089) for 24 major rivers of the world. These papers should be cited in this one.

Answer #1 We introduced references to the papers by Christensen and Lettenmaier (2007), Nohara et al. (2006) and Krahe et al. (2009). Our approach in the case of the Rio Grande is in many ways similar to those studies: we used projections of several different Global Circulation Models to run a hydrological model and evaluated the impacts on streamflow. Uncertainty, in this context, is the wide range of values of this output variable (streamflow) in the future projections. Uncertainty is related to different greenhouse gas emission scenarios; to differing GCM structure and spatial resolution; to downscaling methods; and to different hydrological models used to translate from changes in precipitation and other atmospheric variables to streamflow. Uncertainty related to hydrological models is usually less than from GCMs, and therefore it was not considered in this study. It is true that we could have used a summary of the different GCM projections, for instance the ensemble mean. However, as mentioned by Gosling et al. (2010 - this same special issue), summary statistics such as the ensemble-mean are inappropriate with such projections because "the mean of equal increases and decreases is no change". This effect would be particularly present in the Rio Grande river basin, where half of the GCMs suggest an increase while the other half suggest a decrease in streamflow. Nahara et al. (2006) used a weighted average of the different GCMs using some metric of its ability in representing the current climate as a basis for weighting each GCM projection. However, as stated by Gosling et al. (2010 - this issue): "Forming a single index of model performance, however, can be misleading in that it hides a more complex picture of the relative merits of different models. " The weighting of the GCMs may also have no impact at all. For southeast Australia Chiew et al. (2009) conducted an analysis on weighted and unweighted means and "concluded that there is no clear difference in rainfall projections between the “better” and “poorer” 23 GCMs included in the CMIP3 archive based on their abilities to reproduce observed historical rainfall. Therefore in their analysis, using only the better GCMs or weights to favour the better GCMs gave similar runoff impact assessment results as the use of all the 23 GCMs. For these reasons, in the present analysis, we assumed that all the GCMs are equally credible (although they are not completely independent) but this does require further investigation." Gosling et al. (2010 - this issue).

Comment #2 In Section 3.1 Model calibration and validation (page 6104) the MOCOM-UA optimization algorithm is employed using three objective functions (page 6104 lines 19-21). Despite of these are very commonly used functions, it would be interesting to include in the text a short description of why they were chosen and what kind of flow elements/characteristics are being optimized by optimizing the three chosen functions.

Answer #2 The MGB-IPH was calibrated using three objective functions: volume bias; Nash-Sutcliffe model efficiency for stream flow; and Nash-Sutcliffe for the logarithms of stream flow. These objective functions were chosen as to evaluate different aspects of the agreement between calculated and observed hydrographs. The Nash-Sutcliffe efficiency coefficient is one of the most used metric to evaluate the results of hydrological models, however it puts excessive weight on high flows, and if used alone would probably result in a good agreement for peaks, but a bad result during droughts. The Nash-Sutcliffe efficiency coefficient for the logarithms of discharge values is used because it provides a measure of agreement during low flows. It is less sensible to differences during high flows. Finally, the volume bias is used in order to avoid important differences in long term water balance. Furthermore, volumes are important in this basin because there are several reservoirs which are able to regularize streamflow.

âĂČ Comment #3 At the same Section, but on page 6105 (from line 2), a description is made of how the MGB-IPH model is forced by gridded monthly meteorological data.

Some important concepts are missing in this description related to how data was disaggregated from monthly to daily and the errors included in this step. More specifically, it is mentioned that disaggregation was applied to monthly data to generate daily data following Todd et al. (2010). Disaggregation is an important and sensitive procedure, specially for precipitation, so that the method chosen deserves to be further discussed, including the reasons why to use that specific method.

Answer #3 The MGB-IPH requires climate information at the daily scale. Disaggregation was applied to monthly data to generate daily data using a method described by Arnell (2003), which is further explained by Todd et al. (2010) in the context of the present application. The disaggregation method is based on a stochastic model which assumes daily precipitation follows an exponential distribution, with the coefficient of variation of daily precipitation derived from analysis of available rain gauge data from within each basin. The occurrence of precipitation is described by a simple two-state Markov model with transitional probabilities fixed. We agree that the disaggregation method can be important to the hydrological response. We tried to minimize the impacts by rescaling daily data generated by the disaggregation method in order to maintain the correct monthly total (Todd et al., 2010). We also verified the impact of the combination of CRU data with the disaggregation method by comparing results obtained with this data with results obtained with the actual local raingauge data, and with observed natural hydrographs. Figure 2 of our paper shows a comparison between the observed (natural) stream flow hydrograph, and two simulated hydrographs: the first using local raingauge data and the second using CRU data disaggregated to daily scale. In this figure it can be seen that the hydrograph generated using disaggregated CRU data is not as good as the hydrograph obtained using actual raingauges, however the hydrograph generated with CRU data has a similar range of streamflow values as the observed one, and seasonal behavior is well reproduced. Finally we would like to clarify the reasons why to use the particular method to disaggregate the monthly data. This paper describes results obtained in the framework of project QUEST-GSI. The objective of this project was to assess the impacts of climate change on water resources at catchment scale (see Todd et al., 2010 this issue). Different papers in this special issue describe impact assessments for basins in different parts of the world. To enable a consistent analysis of the climate impacts around the globe, all simulations were done using a unified set of past and future climate scenarios, i.e., all basin partners were provided with a consistent set of historical climate and future climate data for the analysis. In conclusion, we used the same disaggregation method as the other participants of the project, and verified if it had impacts on the simulated streamflows in the Rio Grande. We confirmed that the impacts were relatively low, given our interest in long-term hydrological responses, and considering that the general behaviour of the hydrograph was well reproduced. The text in section 3.1 was changed in order to clarify this issue.

Comment #4 In lines 14-15, page 6105, it is stated that "daily values for the variables used to calculate evapotranspiration were considered to be identical to the mean monthly values." From this phrase, I could conclude that a constant evapotranspiration value is used for each month. If this is actually the case, a serious error is included in the model as evapotranspiration is highly variable. Same is true for solar radiation and relative humidity. As this paper is aimed to uncertainty issues, it is important to discuss uncertainties related to every step of it. Clarification is necessary on how daily evapotranspiration, solar radiation and relative humidity are estimated from the CRU monthly data and what are the implications of such estimations to the hydrological model results.

Answer #4 Daily rainfall and streamflow data in Brazil are freely distributed by the National Water Agency (Agencia Nacional de Águas - ANA). Other daily meterological variables needed to run the model are temperature, wind speed, atmospheric pressure, relative air humidity and incoming solar radiation, or sunshine ours. In Brazil these data are routinely collected by INMET - the National Meteorology Institute - which unfortunately still do not offer daily data for free (on the contrary, they charge heavily for anyone who wants to use the data time series). Therefore we need to use

mean monthly values instead. However, the fact that we used mean monthly data of this variables does not mean that a constant evapotranspiration value was used for each month. This is so because evapotranspiration is also dependent on soil water availability, and this is highly variable due to variability of rainfall. While it is true that potential evapotranspiration is constant for each month, real evapotranspiration calculated by the model is variable. We agree that this may have an impact on model results, and we would like to use daily data to run the model, however we can assure that this impacts are relatively small, due to two main reasons. First, we calibrated the model parameters using the mean monthly data, and obtained relatively good results. Second, we applied the same model in several other river basins in South America (Allasia et al., 2006), with the same data limitation, and obtain relatively good results. In one case (not published) we compared results obtained when using daily and mean monthly data to calculate evapotranspiration, and concluded that model performance worsen only slightly. Therefore we are convinced that the use of mean monthly data for variables related to evapotranspiration does not invalidate the results obtained.

Comment #5 The text states that "Baseline (1961-1990) CRU data were modified so that any trend relating to increasing global mean temperature was removed.". A better description of how the detrend was made is desirable. Some important questions to be clarified are: which variables were detrended? How was the trend determined? How could it be concluded that the trend is actually related to increase in mean global temperature?

Answer #5 We used a detrended baseline 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 detrend 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.

Comment #6 Some points relate to the prescribed temperature increase scenarios deserve some clarification. In line 6, page 6106, it is mentioned "(2) prescribed increases in global mean temperature of 1, 2, 3, 4, 5, and 6 C using the UKMO HadCM3 GCM as well as (3) A1b emission scenario and prescribed warming of 2 C ("dangerous" climate change) using six additional GCMs from the World Climate Research Programme (WCRP)". In Table 1 Obs one can find "+1 to 6 oC over baseline" and "+1 to 6 oC over baseline". What is unclear to me is which baseline this increase in temperature is applied to. Up to this point of the text, the word baseline has been used to refer to the CRU data and I would not think that it is the case for Table 1. Commonly, prescribed temperature increases are applied to the model control run (often 1961-1990). Can it be the case here? If yes, change baseline from Table 1 to control run and stated its period, otherwise, please clarify what has been used as baseline for the generation of the prescribed temperature increase scenarios.

Answer #6 We agree that this part of the text is somewhat confusing. The scenarios of temperature rise of +1 to +6 oC refer to global mean temperature increase related to the 1961-1990 climate. This does not necessarily correspond to a +1 to +6 oC temperature rise in the Rio Grande river basin. These scenarios are a tentative answer to the question: "What would be the climate in the Rio Grande river basin should the average global temperature rise +1 to +6 oC?" We agree that the use of the word baseline in Table 1 is not proper since we use baseline to refer to the CRU 1961-1990 data. Table 1 was changed and the word baseline was removed.

Comment #7 Along the text, it is used the concept of 95% duration flow (Q95) for low flow and 5%duration flow for high flow (Q5). Such definitions are related to the percentile concept. In tables 2 to 5 it is also used the term average river flow that in page 6108 is represented as Q50. At that point I got in doubt if what is referred to as "average flow" is not in fact the "median flow", that is also related to a percentile concept, differently from "average flow" that is related to the "mean"flow. Please, make this clear in the text.

Answer #7 We agree that the term Q50 was used inappropriately. We changed it for

Qmean, referring to the mean flow.

Comment # 8 The first paragraph 1 in page 6104 is a repetition of what is explained in the previous text and so, it can be deleted. Line 8 page 6104: change "station meteorological records" to "records from meteorological stations" Page 6108 line 14: bad location for reference to figure 9. Suggestion: move (Fig. 9) to the end of line 15. Figures 3, 5, 7 and 9 are very small and difficult to see. They can be made somewhat larger. Same for Figure 4 where it is not possible to distinguish the curves related to different models, except if the figure is enlarged.

Answer #8 All the suggestions related to the text have been accepted and the text was changed accordingly. Figures 3, 5, 7 and 9 were increased. Figure 4 is really large, the difficulty in distinguishing the curves occurs because different emission scenarios result in very similar streamflow outputs. We think that figure 4 is important to show how similar the outputs are.

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