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Hydrology and Earth System Sciences Discussions

Interactive comment on "Climate model based consensus on the hydrologic impacts of climate change to the Rio Lempa basin of Central America" *by* E. P. Maurer et al.

Anonymous Referee #1

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General comments:

For practical purposes the paper is interesting. More clarity is necessary in some aspects (detailed in the comments). In the conclusions I would recommend a sentence explaining the limitations of the study, e,g,: possible error sources: regridding/resampling/update (see comments), observed data, the scenarios projections and the model itself) and the uncertainties associated to the projections (your figures 5 and 6).

Detailed comments:

i. In section 3.3: Observed Meteorology: The combination of many different kinds of data source (e.g. Wilmots, New, Shefield, Nijssen, NCEP) and variables (e.g.: prec, temp, u10m) and the different periods, updates and resampling are a bit unclear. Maybe a table with data source, period and the usefulness of the data would clarify what was done with what data and for which period;

ii. In section 4.1: Hydrology model calibration: I got a bit confused by the periods of calibration, validation of simulation, and 'present'(1961-1990), which was, in fact, the period used to compared to the mid and end of the 21st century. Why was the period of calibration chosen from 1970-1979 and of validation from 1980-1989? Was that due to data availability? ;

iii. Still in the same section, you mentioned two dry years: 1983 and 1987. I can see the dry year in the flow data in 1983, but not in 1987. Didn't you mean 1986 instead? What I can see is that in 1987 the bimodal regime does not happen, or the first peak does not appear. Is this the 'reduction'; you mean in 1987? By the way, the years you mentioned 1983 and 1987 (or 1986) are El Nino years. Isn't the studied region influenced by ENSO (please, check http://www.ncdc.noaa.gov/paleo/ctl/images/warm.gif)? Isn't it possible that your model overestimates the flow in these years (mainly in 1983) because during the calibration period you didn't have a strong El Nino like 1982/1983? And all of this comes back again to the question: were these periods for calibration and validation chosen due to data availability?

iv. Still in the same section, you state that 'due to limited data availability no adjustments have been made to the observed flow data to account for upstream diversions or other anthropogenic effects'. Does it mean that the observed data does not contain 'anthropogenic effects'? It not clear. Is this 'limited data availability' also responsible for the choice of the calibration and validation period? If so, I suggest this to be said in the beginning of the section, otherwise I would understand that the 'limitation' would refer only to the 'adjustments of the observed flow';

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v. Still in the same line of thinking and in the same section you say in the final of the section: 'Consequently it is likely that the observed flows used in this study underestimate to some degree the natural flow (excluding diversions, impoundments, or other anthropogenic influences) simulated by the VIC model'. Thinking in the other way round: can't this overestimation from your model be due to data resampling and regridding previously to the VIC integration, although a bias correction was applied? Or, wouldn't it account for errors in the flow simulation and hence leading to the VIC errors more than (or as well as) the errors in the underestimation of the observed flow? Furthermore, I don't see the underestimation by the flow observation happening in the wet seasons in the other years of the validation period (e.g.: 1981, 1982, 1985 and 1988);

vi. In your conclusions, I would be careful with the word quantitative. Although you came up with numbers to quantify the impacts, you have used 16 GCMs, for 2 scenarios and there is an 'error bar' associated with the different models and the two projections, besides the fact you always mention the changes in a range of values. I would rephrase it simply as 'The study provides the assessment of potential changes...' ;

Suggestions for future work (or even for the present, if applicable) are:

- Wouldn't the effects of GHG be more robust by choosing the N best models (could be assessed according to the validation of the simulation period (1980-1989) /present period (1961-1990))'?;

- It is interesting to spot the model differences (i.e. the worst and the best model(s)) that lead to the spread mentioned in figure 4. The same scatter plot could be done to evaluate the models spread containing the model label for the monthly evolution (like figures 5 and 6, but for all the models in the same plot. The mid-21st century and the end of 21st century would be 2 different plots. By knowing the 'best' models and their differences to the others, we would have a hint on, for example, what processes are simulated differently and better, allowing model improvements as well.

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