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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.

E. P. Maurer et al.

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Response to Anonymous Referee 1 (Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 3099, 2008)

Original comments are in regular type. *Responses are in italics*.

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

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Interactive Discussion

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figures 5 and 6).

We are appreciative of the careful consideration the reviewer has given this manuscript. As noted below, uncertainty sources are more clearly discussed in the revised manuscript. Please see our responses to each of the specific comments below for how we revised the manuscript to address each of these concerns or questions.

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.

We added a table (Table 2) that describes all of the data sources for the global meteorological dataset, the period of use, and how each dataset was applied.

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?

A revised second paragraph has been added to section 4.1 to clarify the reasoning behind the 10-year periods used for calibration and validation. This revised paragraph should also help distinguish these periods from the 'climatological'period of 1961-1990 used as the baseline for characterizing future changes.

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

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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?

Thank you for alerting us to this oversight. Paragraph three of section 4.1 has been modified to change the date to 1986. In addition, the paragraph includes a discussion of the occurrence of the El Nino years, the connection of El Nino to the mid-summer drought, and the implications of this on the model calibration.

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'

The fourth paragraph of section 4.1 has been rewritten to make the distinction between the two data streams much more clear. The additional implication of this on the calibration and validation results has also been added to the paragraph.

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

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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);

The last paragraph of section 4.1 has been added to briefly discuss the added uncertainty due to the hydrologic simulations. This section should now much more adequately address the potential sources of uncertainties in the hydrologic simulation.

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...'

The first paragraph to the conclusions has been modified to accommodate this comment.

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))'?

This is an important issue that has driven recent studies. While not the focus of this paper, a discussion is included in the revised first paragraph of section 4.2.

- 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

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simulated differently and better, allowing model improvements as well.

Following the response to the prior question, in the first paragraph of section 4.2 we now refer to the Brekke et al. (2008) and Rauscher et al (2008) references as well. In the latter, they carefully selected three of the 'best' GCMs and arrive at results consistent with ours. The Brekke paper shows that even the determination of which GCM is best can be problematic, and as metrics are added the distinction between 'good' and 'bad' GCMs diminishes. In addition, the resulting estimates of future impacts is not strongly affected by selection of better sets of GCMs.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 3099, 2008.