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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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Original reviewer comments are in black type. *Author responses are in italics.*

Summary: The article investigates the effects of climate change on the hydrology of the Rio Lempa Basin, focusing on the effects of these hydrological changes on the hydropower supply of and inflow to the major reservoirs. In the study an ensemble of hydrological model runs, based on climate forecasts from 16 GCM8217;s, is used. Overall: The structure of the article is clear and well ordered. The study is very practical. However because of the application of an ensemble of climate model forecasts and the number of statistical methods applied for taking into account the GCM uncer-

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tainties, it is also of scientific relevance.

We are grateful to the reviewer for these comments. We respond below to each of the comments below, and indicate where the manuscript has been revised to address the comment.

Detailed: The abstract is a complete summary, already giving the main results.

The introduction poses the study in clear contents, multiple relevant references are used. Also it states the relevance and discerning features of this study.

Description of study area (2), GCMs (3.1) and hydrological model (3.2) are clear and the datasets used are well described and referenced.

The description of the observed meteorology (3.3) is complex, it takes a reader several times reading before understanding. Maybe a table with the meteodatasets used, clarifies this. The choices for the datasets could be better clarified. Why have the monthly datasets of Willmott and Matsuura (2001) and the dataset of New et al. (2000) been used, while daily values were available from Sheffield et al. (2006) and Nijssen et al. (2001)? It is stated that the daily variability is established from the latter sets, has this daily variability been used to downscale the monthly sets in time?

To clarify this, we added a table (Table 2) that described all of the data sources for the global meteorological dataset, the period of use, and how each dataset was applied. This should adequately describe the use of the various observational datasets.

Paragraph 3.4 is clear and the applied method to identify significant change is well chosen.

Results and discussion (4) are clear and the statistic methods are used very well to quantify the results. Figure 4 (4.3) is interesting; however the graph contains less information than the authors have available and that might be interesting for the reader and make the study scientifically more interesting. There is only a distinguishment between the dots into which scenario they belong. However it would also be interesting

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to see which dots belong to which GCM. Are temperature and precipitation changes maybe linearly related within the separate models? Has the ANOVA analysis been applied to the complete set of GCMs at once or have the results of the ANOVA analysis of the different models been combined.

The revised first paragraph to section 4.3 clarifies that our results consider only the relationships among the two sets of 16 GCM projections, and the correlation between precipitation and temperature within any GCM on a month-to-month basis is not evaluated. In addition, numbering has been included in Figure 4 to allow cross-indexing with Table 1 to determine which GCM corresponds to which point.

There is little agreement between the GCM based calculated inflows to the reservoirs in september-december (4.4), is their an explanation for this?

Paragraph 3 of Section 4.4 has been revised to include a more thorough discussion of our findings in this regard. In particular, the changes are smaller relative to the variability among GCMs especially in Oct-Nov, so lower levels of statistical confidence are achieved for the projected changes. Reference is also made to a prior study (Rauscher et al, 2008) that examined the mechanisms driving the differences in projected changes before and after the mid-summer drought.

In paragraph 4.4 it is stated that "the phenomenon of precipitation changes having an amplified effect in runoff, when direct CO2 effects on vegetation are ignored, is well known". It is not well known to me and that may be the reason why I can8217;t follow the next part of this paragraph dealing with CO2 and evaporation. The direct CO2 effects are explained here, although they were well known. The authors concludes that the two CO2 effects cancel each other out. So there is no amplified effect on direct runoff in this case? I suggest rewriting this paragraph, to make it more clear to a reader less familiar with this phenomenon.

The third paragraph of section 4.4 has been rewritten to explain better the concept of direct effects, and how that affects (or does not affect) our interpretation of results.

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