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Hydrology and Earth System Sciences Discussions

Interactive comment on "Coupling statistically downscaled GCM outputs with a basin-lake hydrological model in subtropical South America: evaluation of the influence of large-scale precipitation changes on regional hydroclimate variability" *by* M. Troin et al.

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Bart van den Hurk, Editor Hydrological and Earth System Sciences

Dear Sir,

Please, find attached the manuscript entitled: "Coupling statistically downscaled GCM outputs with a basin-lake hydrological model in subtropical South America: evaluation of the influence of large-scale precipitation changes on regional hydroclimate variability" by Magali Troin, Mathieu Vrac, Myriam Khodri, Christine Vallet-Coulomb, Eduardo Piovano, and Florence Sylvestre that we would like to re-submitted to Hydrological and Earth System Sciences. Corresponding author: Magali Troin.

The initial version of the manuscript has been reviewed by two reviewers. We addressed each comment and question asked by each reviewer. We appreciate the reviewers' comments and suggestions, which helped us improving the quality of the manuscript and strengthened the justification of our approach and scientific results.

We have attached below our detailed response to each major and minor criticism or recommendation.

We hope this paper is now suitable to Hydrological and Earth System Sciences. We thank you in advance for your time and for considering our manuscript.

Yours sincerely,

Magali Troin

Detailed responses to reviewer comments #2

Comments: 1) LMDZ simulations: It is not clear the setup of the LMDZ ensemble simulations: The model was forced only by SSTs? If so, what sea ice was used? Each member differs on the initial conditions of sea ice and SSTs? Does it mean that each member started at different dates (using the same atmospheric conditions)? Indeed we didn't make that point clear enough in the paper but each simulation is forced by HadISST SSTs and Sea Ice and each ensemble member differs by the dates for SSTs and sea ice initial conditions but also from the atmospheric initial condition used for restart. Each simulation starts from a different month during 1950.

2) Downscaling The authors intend to address if it is relevant to force the rainfall-runoff model by downscaled LMDZ and reanalysis output. However, they do not show what would be the results of forcing the SWAT model directly with the GCMs output without downscaling. This could be the benchmark to prove if the downscaling is relevant or not. This is further necessary considering the poor spatial representation of observed precipitation (Troin et al 2010a,b?) As recommended by the reviewer #2, we have used directly the LMDZ outputs in the hydrological model and compared the results with the downscaled simulations (§5.2 and 5.3).

3) Regions/Boxes Why are the regions (for NCEP) definitions different from the boxes (for LMDZ) definitions? The authors should clarify their choice and state that region B is the same as box C. Why is region A so small (only 1 grid-point) when compared with the other areas? The NCEP and LMDZ regions differ since geographical latitudes and longitudes were not the only criteria used to guide our choice, due in part to bias in LMDZ model climatology. Our region selection was also guided by the climatologic structure which is somewhat Âń contracted Âż in the latitude as compared to the observed climatology. For example, the latitudinal extension of the Hadley Cell is a little bit smaller than in the reanalyzes, so that all the structures have a small equator ward bias in LMDZ. In order to obtain comparable quantities, we choose regions in LMDZ that are the most physically and climatologically consistent with the observed physical and climatologic structure. That is the reason why we choose 3 regions in order to test the realism of each region covering the Sali-Dulce Basin. Box C (20°S-25°S;55°W-65°W) correspond indeed to the region B (20°S-25°S;55°W-65°W) from the Reanalyses in term of geographical coordinates and serve as a benchmark for our comparison. Changing the size of the selected region also allowed us to test and explore the impact of the region and size in our downscaling procedure and lake model simulation performance. This approach has been designed to help us test as much as possible the feasability of downaling GCM output to force hydrolocal model by taking into account bias that are inherent (eventhough different from one model to another) to all GCM model.

4) NCEP vs. LMDZ The lake level trends are better simulated using downscaled LMDZ outputs then NCEP reanalysis. These results are more than “An interesting feature” (section 6.2). It is not clear to the reviewer the reasons for these striking results. The authors should elaborate on this topic trying to point possible reasons for this. While the LMDZ simulations can be considered has homogeneous in time the reanalysis is not since different observations were assimilated, leading to different kinds of errors. The PDM approach is suitable for changing climate context since it assumes that there is a mathematical transformation that translates the CDF of the predictor into the CDF of the predictand. In the reanalysis case the PDM will try to correct a CDF that has different sources of errors during the validation period then during the calibration period. This could be one explanation to the better results of LMDZ when compared with NCEP. As stated in point 2) it would be useful to check what would be the results of using the direct GCMs output. We have tried to explain more precisely this point in the discussion (§6.2). As suggested by the reviewer #2, the most logical explanation is probably because our GCM is forced by the HadISST1 data set, which certainly minimizes the source of errors unlike for NCEP/NCAR reanalyses.

5) Lower latitudes areas The best results were obtained with data from regions at lower latitudes than the actual lake catchment. This does not directly suggest that the lake is mainly under tropical climate influence, but that the GCMs and/or PDM over the lake region are not accurate. This is the case, since a SWAT model is used, and it is questionable to force it using downscaled data from a different region then the area of study. This raises a question: Would the PDM or another statistical downscaling method using the same predictands (rainfall, and temperature) predict the lake level directly without recurring to the SWATmodel? Testing and comparing several statistical downscaling method is a very interesting question even though it is out of the scope of the present study. It would be also very interesting to see what would be the result of using a fully statistical approach to simulated and predict directly the lake level without considering hydrological processes as we did by using SWAT. But the reviewer would probably agree that this would be an alternative method from the one we choose to

develop in the present paper and would constitute a different study all together. We actually agree with the reviewer concerning the fact that the best results are obtained by using LMDZ box B and C does not necessarily prove a dominating tropical influence. We didn't phrase correctly our point concerning the conclusions we draw from these results and corrected that point in the text. We think that this result tend to suggest that the source of variability that force and characterize the simulated evolution of the lake level are obtained when using downscaled precipitation and temperature from the northern border of the actual catchments area. Therefore this suggest that the precipitation/temperature that feed most realistically the lake level variations comes, from tropical regions as previously shown by Troin et al 2010 who used rained gauged stations and observed river runoff to simulated the lake level variations. Troin et al (2010) study revealed that the tropical source for the runoff and precipitation in the northern part of the basin was the one controlling significantly the lake level fluctuations.

Technical corrections: Pag 9536 after eq(2)The observed runoff and mean observed runoff should be referred in the text as Qo (and not Qs) Done.

Pag 9536: The last term of equation (3) (gamma) is not defined We have defined the gamma in equation 3.

Pag 9546: There are two references as Troin et al 2010. The authors should include a, b and change in the text for the appropriate reference. Done.

Figures 3-6: The authors should increase the resolution and size of the figures. For example, figure 6 can only be clearly analyzed when zooming to 600%. When printed it is difficult to examine the figures. We have increased the resolution and size of the figures.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/7/C5191/2011/hessd-7-C5191-2011-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 9523, 2010.