

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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Dear authors

Although you have revised your manuscript quite drastically particularly by reducing the number of figures and tables, I still feel that many of the identified shortcomings have not adequately been addressed in this new version. I have decided not to reject the

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paper at present, but to send it out for review again after you have been given a next (last) opportunity to improve the manuscript in which a number of major shortcomings I still have identified have been adequately addressed:

* The paper describes and illustrates the added value of downscaling/correcting re-analysis/GCM generated precipitation/temperature data used to force a hydrological and a lake level model in SESA. However, the presentation of this added value as concentrated in tables 3-5 and figures 4-5 is not adequately presented. The tables do not contain a clear indication of the improvements obtained by the downscaling procedure since a comparison between uncorrected/corrected data is not given. Figures 4 and 5 display long time series where a systematic offset between the curves are clearly identifiable but where a clear added value of the downscaling (apart from a general offset correction) is not obvious. A more informative way of demonstrating this added value is mandatory.

* The method applied is named a "statistical downscaling" rather than a "bias correction" procedure, although the discussion spends some words to the semantic differences between these two procedures. However, the statistical downscaling uses large scale precipitation/temperature input data, but it is unclear why you selected these fields as predictors, and not features related to the large scale meteorology and moisture advection such as Z500 or moisture convergence. Do the chosen predictor fields explain more variance of local variables than another selection of large scale predictors?

* The selection of the boxes and regions is not well motivated. You state that these areas are "most physically and climatologically consistent" (L246) but this seems as a rather arbitrary choice. Did you apply some kind of objective method to verify that these regions are indeed optimally selected?

In addition I have added a list of specific issues that I have noted while reading your revised manuscript:

L26: "region" -> "regions"

L28: insert: "bias correction and"

L38: "that force the most" -> "that forces most"

L50: insert "the" before "Southeastern"

L65: delete "out"

L79: "seemed" -> "seem"

Figure 2 is still redundant, unless you make specific links between the results of the study and the processing steps displayed in this figure.

L80: I tend to disagree: not only the limited spatial representation of local hydrological processes cause a bias, also the lack of generating adequate large-scale forcings to the local climate variability play a clear role. And this is not only due to limited model resolution.

L84: "This mismatch, between..." -> "The mismatch between..."

L88: insert "bias correction and" before "downscaling methods"

L105-106: I don't understand "some interest ... such as computation efficiency". Is this efficiency an interest?

L236: still unclear that the raw/corrected NCEP/LMDZ data sets are actually the 4 scenarios that you have developed.

L382: "day" -> "days"

Caption table 3-5: unknown what a "Downscaling method calibration fit" is. Please rephrase caption starting with the actual variable that is listed (Explained variance).

L398-399: "...the PDM performances remained acceptable (Table 4/5)": how can one conclude this from this table? What is the (objective) benchmark to which one can

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compare the numbers in these tables in order to judge that the performance remains acceptable?

L425: "The best results were found for region A". Is there a reason why this can be the case?

Fig 5: legends have not been added

L463: "A clear opposite trend" is not very obvious to me. Rather one can see a systematic offset between the raw and downscaled time series with only occasionally (during small episodes) different trends

L490: "the relative merits": these are not at all obvious from the results presented in tables 3-5.

L511: there is no fig 6 anymore

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

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