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# *Interactive comment on* "Assessing Water Security in the Sao Paulo Metropolitan Region Under Projected Climate Change" *by* Gabriela C. Gesualdo et al.

**Anonymous Referee #2**

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First of all, I express my deepest apologies to the authors for this late review. The manuscript presents projections of the future evolution of the surface water resource in the Jaguari river Basin, the main water supply area for the Sao Paulo metropolitan region and the corresponding evolution of some proposed water scarcity indicators, depending on the evolution of the demand. The presented approach is relatively classical. It combines a calibrated hydrological model (Hymod) and an ensemble of 17 stochastically downscaled GCM model outputs for two contrasted climate change scenarios (namely RCP 4.5 and RCP 8.5), to generate streamflow projections at the outlet the considered watershed for the period 2010 to 2095. The manuscript is well written and structured, sound and pleasant to read. Little is said about the hydrological model



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(section 2.3) and the GCM output downscaling procedure (section 2.4). This simplifies the presentation and makes the manuscript easier to read. The readers are referred to previously published papers for more details. The results of this projection work, mainly presented in figures 4 and 5, are surprising and insufficiently discussed and commented. In fact, all projected average monthly streamflows appear very similar for all periods and the two scenarios (RCP 4.5 and RCP 8.5) and differ significantly from the actual situation (fig. 4). Such a little contrast between RCP 4.5 and RCP 8.5 is difficult to understand, especially for the second half of the 21st century where both projections differ greatly for the evolution of temperatures, which have a direct impact on potential evapotranspiration. This extremely strange outcome is acknowledged by the authors (P8, L3-5) but not explained nor discussed. As the rest of the manuscript and the conclusion are based on these results, a critical analysis appears to me as essential. For a better insight, the authors should at least present:

1) The projected evolutions of temperatures and potential evapotranspiration for all periods and scenarios to be added in figure 4. This will certainly reveal clearer contrasts between scenarios.

2) The GCM simulations for average monthly temperatures and rainfall for the actual period should also be presented. A major concern in climate change studies, especially when rainfall is considered, are the intrinsic biases of GCM models. A large amount of works have been devoted to the treatment of these biases to provide reasonable trends. Nothing is said about this problem in the manuscript and I highly suspect that the major differences between actual and projected situations, that draw the attention of the authors and on which their conclusions are focused, may be mainly due to these biases. If this is confirmed, the conclusions of the manuscript should be considered as invalid. This missing discussion and treatment of climate projection biases is a real major flaw and made me hesitate very much between suggesting "major revisions" or "rejection". It should absolutely be solved in a revised version of the manuscript.

I add some minor comments:





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1) Since the manuscript is mainly focused on low flows, criterions specifically focused on the lower flow values should also be used to assess the hydrological model. R2, MSE and KGE are predominantly controlled by the larger discharge values.

2) The precipitation unit must be clarified in figure 4 (mm/day)

3) The figure reference numbering does not seem to be correct at some places in the text (4 rather than 5 at op8 L4 and P9L7).

4) At P7L25: it must be specified that the authors speak about "hydrological dryness". The rainfall amounts start to rise in October and November even in the projections.

5) P7L24: It cannot be stated, based on the presented results that rainfall extremes increase. The authors only present monthly averages. In general, the authors should avoid presenting conclusions that are not directly related or illustrated by the presented results. In the same line of thought, plant water stress mentioned on P8L2 should be illustrated (through the simulated soil water contents for instance). By the way, how is the vegetation cover reaction to the climate change taken into account? Again, it is suggested that RCP 8.5 generates more intense rain : please illustrate this fact based on the available projections. P9L20 : There is no direct relation between the increase of extreme rainfall and the possible increase of monthly rainfall in December. As for the previous remark, if it is true that the projected rainfall amounts are linked to an increase of the frequency of extreme events, this can be illustrated based on the climatic projections.

6) Figure 5 increases dramatically the undetectable contrasts of figure 4. Why? Moreover, some inconsistencies seem to exist between the two figures. If the demand is considered as relatively constant over the year (if it is not, this should be explained and commented by the authors), discharges and scarcity and vulnerability indicators should have the same dynamics. It is not the case. The lowest simulated discharges are observed in October for all scenarios and periods (fig 4) ; Why are then peak indicators values computed in November? Some explanations are clearly missing.





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7) Figure 6 is not needed since the same results as in figure 4 are presented, except that error bars have been added. It is by the way not explained how these boxplots have been build. Do they represent the inter-annual variability (this is what I suspect)? Or do they represent the variability of the projections of the 17 tested GCMs. By the way, these 17 simulations and the information provided by the variability of their outcomes are never presented nor used in the manuscript. This should be added somewhere.

8) Section 3.4 is not really related to the rest of the manuscript. These thoughts about ˆpublic policies are not totally uninteresting, but not supported by the presented results. In would suggest to remove this part, or to summarize it in the conclusion of the manuscript.

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