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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assessing residual hydropower potential of the La Plata Basin accounting for future user demands" *by* I. Popescu et al.

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We would like to thank Prof. Castellarin for taking time in reading and suggesting modification to the paper. We found all comments very useful to improve our paper.

Answers to the comments are as follows:

Comment 1: General comments: *The manuscript main aim is to quantify the maximum and residual hydropower potential of the whole La Plata Basin (LPB, area: 3500000 kmËĘ2; population over 200 millions) and compare it with the current hydropower production and electric energy in the region and estimated future energy*

demand (i.e. year 2040). The study adopts tools that were previously developed to perform the estimation, therefore it is not innovative in this sense. Nevertheless, given the geographical scale of the analysis, the hydrological relevance of the study region, and the topical issue addressed, in my opinion the manuscript presents a very important piece of information which is definitely of broad international interest. It also raises some serious questions on how to manage in a sustainable way the future energy demand of the area, which cannot be fulfilled by hydropower production only. I really enjoyed reading this manuscript, which is neatly organized and which I believe is worth publishing in HESS. I only have a few main remarks and some minor comments on this manuscript that should be considered while revising the manuscript for improving the clarity of the presentation. I detail them below in the hope that the authors will find them to be useful while revising their manuscript.

Authors' answer: We would like to thank the reviewer for taking time and evaluating the paper in this way. We are indeed glad to follow the suggested changes so that the manuscript is improved in readability and clarity. Please find our answers bellow to the specific comments.

Comment 2: Climate and hydrological variability *Reading the manuscript I got the impression that natural climatic and hydrological variability of the study region is only marginally accounted for. Once detected positive change points in the mean of annual flows for a large number of gauging stations in the region around year 1970, the authors discarded all observation predating the change-point year and focused on observations collected in the last 40 years. The authors could better discuss in the context of environmental change (see e.g. Barros et al., 2006) the reliability of this assumption for estimating the (maximum and residual) hydropower potential, which is used as a reference also in 2040.*

Authors' answer: The authors would like to thank the reviewer for raising this issue. Indeed as pointed out by Barros et al, 2006 during the seventy's decade intense deforestation took place in the LPB, giving room for agriculture. These changes have contributed to the change in evapotranspiration and surface runoff, which increased

HESSD

9, C2703–C2714, 2012

the meanannual flow. These changes have been detected by the downstream gages located on the Parana river. Further we are looking at year 2040, which is a close horizon, and this is why we did consider, in view of the facts presented by Barros et al that the last 40 years of data are representative for the study. We will add this remark in the revised version of the manuscript.

Comment 3: *Also, hydropower potential is evaluated on the basis of the local mean annual flow. Nevertheless inter- and intra-annual variability of streamflow may be very significant (and even under the hypothesis of stationarity of streamflow regime, see e.g. Castellarin et al., 2004 and 2007), particularly so in monsoon areas. On top of this, non linear relationships exist between streamflow and hydropower production (see e.g., Vogel and Fennessey, 1995), together with threshold effects (e.g. hydropower production may be hampered during major floods). I believe that a discussion on these points and how they impact the accuracy of the estimated hydropower potential would enrich the article, also given the hydrological readership of the Journal.*

Authors' answer: As rightly pointed out by the reviewer, we are acknowledging the fact that we did not take into account the inter-annual variability and used the mean annual flows It is an assumed simplification of the study, because indeed there is not an unique and linear relationship between water availability and hydropower production (it strongly depends on the rainfall distribution along the year and storage capacity of reservoirs). On the other hand, the intra-annual variability in our analysis is partially taken into account, because we used about 3 decades (mean 70's to mean 2000's) of data. Also, we are working in a very large catchment, and it would not be possible to analyse the entire area, unless we make simplifications as we did (constant mean discharge over the year). This is certainly a first approximation. Looking to smaller areas (sub-catchments), where there are not enough gauges, the analysis should be done at least using the discharge duration curve and the analysis presented in the papers pointed by the reviewer. We will make our assumptions clear in the manuscript while presenting the methodology and suggest in the conclusion part how to further improve the results by looking at smaller catchments and address them as presented

HESSD

9, C2703–C2714, 2012

in the work presented in the papers that were advised to us.

Comment 4: *Uncertainty of the estimated 2040 scenario: The authors should make an effort to strengthen the discussion on the reliability of the proposed estimates for year 2040 (see also specific comments), concerning in particular the evolution in time of the energy demand. Results of their investigations are presented in a deterministic fashion, while the analysis involves a number of assumptions and sources of uncertainty. In Fig. 8 the authors reports the CLARIS-project estimates of the energy demand, which are characterized by upper and lower limits. While the main message of the paper still holds (i.e., the future energy demand cannot be satisfied by means of hydropower production only, no matter what) and its strength is not impacted, the upper and lower scenarios provide the reader with a fairer picture and a rough idea of the uncertainty of the estimates.*

Authors' answer: We thank the reviewer for this valuable comment. Indeed we followed a deterministic approach to estimate the energy demand. We will add in the revised manuscript, in the conclusion part the issue raised here, and explain what are the limitations of the assumptions we have made in the paper and how a further study may be improved and should address the uncertainties that are introduced by different assumptions.

5: *Suitability of the Vapidro-Aste tool. The GIS tools adopted in the analysis was developed in a different climatic and geographical environment and, perhaps, made a direct reference to a particular hydropower production scheme. I would encourage the authors to discuss the suitability of the tool in the context of this study.*

Authors' answer: The theoretical equations used in VAPIDRO-ASTE are universally valid, they are not dependent on the region where they are applied, nor are there any parameters included in the tool that re specific to a certain region. Our challenge was to apply that methodology of VAPIDRO-ASTE to such a large area as LPB is.

Comment 6: *SPECIFIC REMARKS*

6.1. *Abstract (and p.5640, line18): "maximum available water in the catchment",*

HESSD

9, C2703–C2714, 2012

please reword or clarify.
Authors' answer: Indeed the wording is not clear so we are suggesting the formulation as follows: "deduced from the available water in the catchment (estimated based on measured hydrographs of the past years)". We will include this formulation in the new version of the manuscript.

6.2. *"1970-2000", or 2010, please check.*
Authors' answer: Thank you for telling us, indeed we checked, it is a typing mistake. It is indeed 1970-2010. We corrected the mistake.

6.3. *p.5636,line 24-26: "it has low impact on the environment", I believe it depends on several factors, e.g. presence and size of the reservoir.*
Authors' answer: It is indeed right, we wanted to say that the process of producing the energy is environmental friendly and is not producing pollution. Indeed if the reservoir is big, the impact on the environment, from physical point of view is huge. We therefore have rephrased as: "it has low pollution impact on the environment;"

6.4. *p.5638, line 12: 2010 appears twice*
Authors' answer: Correction done.

6.5. *p.5638, lines 18-19: Is "major hydropower development" appropriate for a 200MW development shared by 2 countries? Please, check. Related to this, the Introduction could better clarify what the main focus is between large, small or micro project, and dammed or run-on-the-river project.*
Authors' answer: There is a typing mistake, the development is of 2000 MW, referring to both Paraguay and Argentina. Further to the comment we will introduce on page 5640, line 20, the following concept: "The maximum and residual hydropower potential of the basin, available for large –small or micro projects, are assessed for the mean annual flows of the present hydrological regime (1970-2010) and topographical characteristics of the area."

6.6. *p.5639, line 7: Alterach et al. (2008a,b): there is just one entry in the reference*

HESSD
9, C2703–C2714, 2012

list
Authors' answer: We have corrected to Alterach et al. (2008).

6.7. *p.5639, line 11: Contextualize acronym LPB also in the body of the article*
Authors' answer: Indeed though we have done it in the Abstract we did not do it at the first occurrence in the body of the article. We have done it now, on p5639, line 11, in the new version of the manuscript.

6.7. *p.5639,line 27: Consider dropping potential*
Authors' answer: We have dropped the word "potential" from the phrase. Thank you for the suggestion.

6.8. *p.5639,line 29: "some factors", please clarify*
Authors' answer: We referred to the fact that land could change its use from agricultural to urban and/or may be lost due to construction of large reservoirs. After carefully re-reading the phrase we had dropped the term "some factors", because urbanisation covers this aspect, and we have introduced the explanation that land could be lost due to construction of large reservoirs.

6.9. *p.5640,line 2: Add "World" before "Water"*
Authors' answer: Correction done.

6.10. *p.5640,line 13: "accurate", how accurate is the estimation? An assessment should be provided (see also my main comments)*
Authors' answer: In this phrase we did not look into details of how accurate the estimation is, but on the fact that the advent of computers are helping us in using models to achieve good representation of the hydrology of a catchment and of the river flows. In order to be clear with this we have rephrase the paragraph, as follows: "The advent of modern computation tools, such as geographical information system (GIS), remote sensing and hydrological models, can support us in making accurate estimation of river flows and water availability in a particular section of a river (Ghicamo et al, 2012). Based on these flow computations, the estimation of the hydropower potential

HESSD
9, C2703–C2714, 2012

is improved as well."

6.11. *p.5641,lines1-2: Can the authors anticipate the definition of maximum and residual hydropower potential somewhere around here? They both appear only later in the text. Related to this, the Introduction could better clarify which is the main focus of the study between large, small or micro projects, and dammed or run-on-the-river projects.*
Authors' answer: As recommended we have introduced the definition of the maximum and residual potential in section 1, at the end, just before the overview of the paper content, and before p5641, lines 1-2.

6.12. *p.5641,line 6: "emans", please check*
Authors' answer: It is a typing mistake "emans" was corrected in "means"

6.13. *Equation 1: Please illustrate all terms (rational formula terms are not introduced). "Conv" is not dimensionless, please indicate units, I also believe that it includes water density. Consider replacing ; before Hi with and, since they both refer to elementary area i.*
Authors' answer: We have added the explanation to the missing terms and eliminated the ";" as suggested. We have verified and Conv as being a unit conversion factor to transform the energy in GWh/year. It's value is computed as 24*365*10-6=0.00876.

6.14. *p.5643,line 5: MIF computed as 10% , please support this choice (e.g. reference) Unclear if i and j represent cells along the river network or not*

Authors' answer: The fact that MIF is computed as 10

6.15. *Section 3.2: Please better clarify how these test were used (e.g. confidence intervals) and provide references.*
Authors' answer: The references to the test, and brief explanations on how these tests were used will be added to the new version of the manuscript. We have added the following text: "Long timeserie data availability of discharge records at hydromet-

HESSD
9, C2703–C2714, 2012

ric stations in LPB point to the need of checking for inhomogeneities, which could be caused by changes in measurement procedures and techniques or even relocation of the observation stations. There are numerous statistical methods available to evaluate the homogeneity of monthly to annual climatic time series. The three most used test methods to determine the start of homogeneity in a set of time series are: the standard normal homogeneity test (SNHT) for a single break (Alexandersson, 1986), the Buishand range test (Buishand, 1982) and the Pettitt test (Pettitt, 1979). All the above mentioned tests assume under the null hypothesis that the annual values Yi of the testing variable Y are independent and identically distributed, and a step-wise shift in the mean (a break) is present. These three tests are capable of locating the year where a break is likely to appear. All these three tests were applied in LPB, because although all three have many characteristics in common, they have different advantages as well. The SNHT detects breaks near the beginning and the end of a series, whereas the Buishand range and the Pettitt test are more sensitive to breaks in the middle of a time series (Hawkins, 1977). In the SNHT and the Buishand test Yi values are assumed to be normally distributed, while in the Pettitt test this assumption is not necessary."

6.16. *'split', consider using 'change point'*
Authors' answer: We have made the correction in the text.

6.17. *Fig. 2, include gauging station locations, if possible.*
Authors' answer: We will add a new figure, or on the existing figure 1, the location of the gauges.

6.18. *p.5644,lines 24-23: 'The explanation for this change. . . after 1950s in LPB', Barros et al. (2006) include also changes and alteration of rainfall regime as possible explanation.*
Authors' answer: We have added this information to the new version of the manuscript. Thank you for pointing this out to us.

6.19. *p.5645,line10: Vapido, please check*

HESSD
9, C2703–C2714, 2012

Authors' answer: Correction made. Thank you.

6.20. *Exponential vs. Logistic: I do not believe that there is the need to refer also to the exponential distribution as: (1) the population growth will eventually level out and, more importantly, (2) the initial stage of the logistic function is approximately exponential, it is therefore a matter of parameterization.*

Authors' answer: We recognise the problem posed here, because at the beginning of the research we looked on which models to identify for population growth. The main difference between an exponential growth and a logistic growth model is that the exponential growth model has a rate of growth proportional to the existing population at a moment in time, while the logistic growth model has in it a damping factor as well, and does not grow at the same rate as the exponential model. Based on previous years census we have tried both models for the LPB countries, and we obtained that the logistic model was appropriate just for Brasil, not for the other countries.

6.21. *p.5647,lines 16: 'Most of the cultivations . . . using rain water', will it be like that in the future also?*

Authors' answer: Yes, the same crops and use of rain water will be valid in the future as well.

6.22 *p.5648,line 2: 'crop I', check capital i*

Authors' answer: We made the correction.

6.23. *p.5648,lines 10 and 18: Is PG the population (eq. 10) or the population growth (eq. 11)*

Authors' answer: In both eq (10) and (11) PG is the population growth. We have made the correction in the text.

6.24. *p.5649,bottom line: Fig. 9 is probably Fig. 8.*

Authors' answer: Thank you. We made the correction.

6.25. *p.5650,lines 5: Please check 'hast' and 'encapsulates'*

HESSD

9, C2703–C2714, 2012

Authors' answer: It was a typing error. We made the corrections.

6.26. *p.5650,lines 11-13: Consider anticipating the definition of residual hydropower potential (see also comment above)*
Authors' answer: Thank you. We have addressed this. Please see the answer to your previous comment.

6.27. *p.5650,lines 15-18:Reference is made to mean annual flow only in the computation of hydropower potential, please comment on the accuracy (uncertainty) of this estimation.*
Authors' answer: The maximum hydropower potential is defined as the total possible hydropower production, considering that all rainfall water, from the location where it drops in the ground till the closure of the catchment, can be used for HP production. Therefore, to consider the mean annual flow is correct (to consider the mean duration curve should give the same result).

6.28. *p.5660,last paragraph: Please justify choices of reference values, or provide references (MIF, efficiency factor, loss coefficient, etc., and also cross-section distance of 50m and 250m on p. 5651)*
Authors' answer: The main aim of the study was to determine the maximum hydropower potential, but also to be able to compare the obtained result with previous studies in the area, such as Claris project. The selected values were in accordance with the values considered in Claris project. Indeed in the conclusion we could propose that a further study would look into the sensitivity analysis of this study to different parameters.

6.29. *Section 5. Consider rewording and clarifying 1st sentence*
Authors' answer: We have rephrase the sentence as "In case that the maximum and residual hydropower potential of the LPB is computed in detail along the river reach then the obtained values are 354,134 MWh and 307,034 MWh respectively, whereas if they are computed, just at the outlet of the sub-basins (i.e.. at "basin scale"), these are

HESSD
9, C2703–C2714, 2012

829,202 MWh and 715,602 MWh, respectively.', which is indeed more clear than the previous one.

6.30. *Conclusions: The last conclusion appears for the first time in the text, I believe it is interesting and should be deepened further and described in more detail, if possible.*
Authors' answer: The level of representation of the river reaches in the GIS environment in the work presented in this manuscript is that of 12 levels of tributaries, while a previous study, was based on a 5 levels of tributaries and did not take into account water withdrawals in the basin. We will clarify and explain this in the conclusion part of the manuscript, as well as we will give references to the previous work done. (Palomino Cuya D.G. et al., A GIS-based assessment of maximum potential hydropower production in La Plata basin under global changes, Renewable Energy (2012), http://dx.doi.org/10.1016/j.renene.2012.06.019)

6.31. *Table 1: Please, consider rounding values at 10ËĘ3. Also it appears that these value do not agree with Fig. 5 (see e.g. Brazil), but perhaps I missed something. Please clarify. Table 2: Please report units Table 3 caption: Revise 'rein'*
Authors' answer: We will make all requested corrections on the revised version of the manuscript.

6.32. *Fig. 1: It would be great to highlight streamgauges used in the study in the main window and LPB in Latin-America small panel Fig. 2: Please indicate streamgauge and its location, timescale of the hydrograph, legend does not seem match with diagram, meaning of the labels Dry, Wet Mean is unclear. Fig. 3: Indicate the streamgauge Fig. 4: I do not think that naming every other streamgauge makes sense, name them all or none (and perhaps provide locations)*
Authors' answer: We will make all requested corrections on the above mentioned figures, as requested, in the revised version of the manuscript.

Fig. 5: Please, increase font size. Logistic fitted on Brazilian data does not seem to match the data properly, please explain. Also, Brazilian data seem to indicate a

HESSD
9, C2703–C2714, 2012

change in concavity (that can be described with a Logistic function) starting from the '70s, that would have a strong impact on the estimates for 2040. Fig. 6, 7 and 8: Please, increase font size. I was struck by the strong linearity of your estimates, when the main driver in the study is population growth, which is non linear in the predictions (see Fig. 5). Please, could you comment on this and on the overall uncertainty of the estimates presented in the figures for 2040.

Authors' answer: Thank you reviewer for the indication that the figures are not clear and readable. We will increase the font size and where required (due to the size of population in different countries), we ill introduce a new figure (split figure in several figures, for clarity). As per the last comment we did work with linear relationships.

Authors' final remark: All our responses to the questions raised by the prof. Castellarin will be included in the revised version of the manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 5635, 2012.