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Interactive comment on "Reservoir storage and hydrologic responses to droughts in the Paraná River Basin, Southeast Brazil" *by* D. C. D. Melo et al.

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General comments

This paper on drought dynamics in the Paraná Basin, Brazil, by Melo et al. draws on a number of data sources, especially from remote sensing and global modelling, that were not accessible just one or two decades ago. Together with the operation data from most of the important reservoirs of the area that is now easily accessible via Internet, the authors took the chance of providing new insights on how the hydrological system and the anthropogenic water management within that region reacted to the drought phases of the recent years. However, the assessment doesn't quite match the expectations raised by the Introduction touching on subjects like the water crisis in São

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Paulo, types of droughts and their propagation through the hydrological system, social impacts, and hydroelectricity generation.

The authors present practically two analyses which are only loosely connected. First, some of the collected data about hydrological fluxes and storages in the Paraná Basin are presented, and two drought phases and some general patterns of the hydrological system behaviour are identified. Second, 37 reservoirs are grouped by hierarchical cluster analysis of their water storage dynamics; the clustering largely depends on the proportion of natural vs anthropogenic influence regarding their inflow dynamics.

Much more of the potential provided by the data could and should be tapped. My principal suggestions for an overhauled version of this paper would be:

- Close the hydrological balance. There are data on precipitation, evapotranspiration, and runoff (principal fluxes), and you have soil moisture, reservoir and total water content alterations (principal storages). The only principal storage missing is groundwater, but it should be possible to calculate it as residual difference. The groundwater component could also be approached from the dry weather discharges. And there are other redundances, too, for example two precipitation and two evapotranspiration data sources. Even without modelling, it should be possible to determine whether the numbers are generally “adding up” or where there are larger uncertainties.
- There are only a few, general points made on the propagation of the droughts from the meteorological via the natural-hydrological and the water management system into the societal system. This should be much more elaborated and ideally also exemplified. Research questions could be about the different onset times of a drought (meteorological, hydrological etc.), or which information is currently evaluated for the water management (situation in upstream reservoirs? weather forecasts?) and what your study can do about it. It is not clear to me yet how you “establish a comprehensive understanding of the linkages between meteorolog-

ical and hydrological droughts for future management" as you advertised in the Abstract.

- The cluster analysis of the reservoir storage dynamics is an interesting view on their operation patterns. But the actual dynamics of single reservoirs over time, ideally with some ideas or information about the decision processes of the dam operators, should also be considered for discussing future management options. One open question to the reader of your current manuscript is: Did the reservoir system sufficiently buffer the drought effects or have there been stress situations (e.g. throttling of power stations) that could have been avoided by better management?

Perhaps you needn't address all of these suggestions for this one paper, but I think it's really worth to dig a lot deeper into the basin processes than you did so far.

Regarding the aspects listed in the HESS review criteria page, my opinion is:

1. Yes, the scientific questions raised are within the scope of HESS.
2. Yes, the paper presents new data, and probably also a new idea regarding the application of the hierarchical clustering.
3. No, the conclusions don't seem to be really substantial at the present stage.
4. Yes, the scientific method and assumptions are valid and (mostly) clearly outlined.
5. Yes, the results are sufficient to support the interpretations and conclusions.
6. Generally the description of the research is detailed enough to allow for reproduction; there are only few exceptions (e. g. some details of the clustering method).

7. Generally the authors always give proper credit to related work; one questionable passage of probably external origin is indicated in the Specific comments section below.
8. Yes, the title clearly reflects the contents of the paper.
9. Generally the Abstract provides a concise and complete summary but also advertises an importance of the work that cannot be matched.
10. The overall presentation is relatively well structured and clear.
11. The language is mostly fluent and precise.
12. Mathematical formulæ, symbols, and units are correctly defined and used, but the abbreviations need more care. There are a lot of acronyms introduced many of which are rarely used again, sometimes they are used inconsistently, or there should be a second clarification of a meaning after several pages of text without the acronym in question while there are also unnecessary double explanations.
13. Yes, one part of the paper should be reduced while others should be shifted from the Supplement into the main text. For details see the Specific comments below.
14. Yes, the number and quality of the references are appropriate.
15. I cannot judge on an appropriate amount of supplementary material. For my taste, it wouldn't be necessary to explain well-established methods (like the Mann-Kendall trend test) once again; there's enough textbook literature. The quality can however be much improved by deleting unnecessary repetitions and a few language touch-ups.

Regarding the considerable data collection and the importance of hydrological understanding for the water management in the Paraná Basin my general recommendation would be to publish the paper after a "super sized" major revision.

Specific comments

Practically all the numbers given are prepended with a math tilde ($\sim$) signalising uncertainty. The heavy use of this feature clutters up the text and doesn't help much with the interpretation. As hydrologists, we are all aware of the ubiquitous uncertainty in our research field, so I suggest to largely delete these little distractions except in cases where you have only got a really rough guess. (And there, $\approx$, made by `$\approx$`, would be the preferable character.)

According to the Abstract, the Paraná Basin holds 70 million people, on Page 2, Line 15, the same number is given for the whole Southeast Brazil, and on Page 3, Line 26f, 60 million people live in the basin. It would be great to not only have the correct numbers but also their source.

A similar issue exists with the percentage of the basin inhabitants in the Brazilian population: 32 % (Page 1, Line 3 and Page 3, Line 27) or 65 % (Supplement Page 3, Line 6f)?

Page 1, Line 9: Which one of the (at least two identified) drought events do you mean here?

Page 1, Line 17: How were the dollar amounts adjusted to 2012 dollars? Did you use the official Consumer Price Index (CPI)? Please explain.

Page 1, Line 19: No need to repeat "(adjusted to 2012 $)" here.

Page 3, Line 2: I would rather write "How are different reservoirs operated under drought conditions?", because the reservoirs are not autonomously reacting.

Page 3, Line 13: I would associate "regional reconaissance" rather with a spy mission charting an enemy territory than with the blurred Earth view of the GRACE data. Maybe you can find a better term.

Page 3, Line 22: The research domain area seems only vaguely known to the authors

($\sim$ 800,000 km^2) although they obviously used a GIS for making the maps. It should be easy to have a more precise figure here.

Page 3, Lines 22 and 29ff: First, there seem to be 35 reservoirs in the Paraná Basin, then there are suddenly about 50, of which 37 are considered for the study. Please clarify. (According to what I see from the rest of the material, there are obviously more than 50 reservoirs within the basin, but you acquired the data of 50, including two reservoirs outside the basin. Finally you decided to study only 35 reservoirs within the basin and the two outliers. Is that correct?) The decision process that caused the reduction in the number of investigated reservoirs should also described in detail here. There are some hints in the Supplement, but your criteria for keeping or dropping single reservoirs remain unclear.

Page 4, Line 1f: How many reservoirs belong to the Cantareira system and what is their overall storage capacity? How much storage volume is assigned to one inhabitant of São Paulo?

Page 4, Lines 10ff: The passage from "Standard GRACE spherical harmonic processing. . . " to ". . . solutions to match outputs from land surface models spatially" reads as if the respective GRACE data handling had all been your work. Is this true? If not, please make clear which parts of the data preparation were already included in the product you obtained from the University of Texas CSR.

Page 4, Lines 21ff: Which precipitation dataset did you use for calculating the SPI, P_{obs} or P_{Sat}?

Page 5, Line 1: When applying hierarchical clustering, several decisions have to be taken. For instance, the distance measure or the cutoff height. The result can be quite different when you alter these parameters, so you should explain explicitely what your choices were and why you made them. Beyond that, it remains absolutely unclear how the locations in the virtual clustering space were derived from the reservoir data. A clear picture about both points is essential for the methods description; this cannot be

pushed away into the Supplement.

Page 5, Line 5: I would suggest renaming “Results” to “Results and Discussion”, because this is not clearly separated.

Page 5, Line 13: Having been introduced to TWS on Page 4, Line 9, we have suddenly TWSA here. And from here on, TWS and TWSA are used interchangeably. As far as I understood the GRACE method, TWS can never be measured in its absolute quantity, only its alterations/anomalies, which is obviously the meaning of TWSA. Please use either TWS or (better) TWSA uniformly in your paper to avoid confusion.

Page 6, Line 2: This is probably the only place where the repetition of an acronym explantation would make sense: RESS was introduced three pages above and is mentioned here for the first time again.

Page 6, Line 7f: Why are you so cautious here? The discrepancy *is caused by* further depletions in deep SMS *and* groundwater storage! What else could explain it? In the Supplement, you show at least four receding groundwater levels (Figure S7), I think this is more than obvious.

Page 6, Line 13: “. . . below the equivalent system maximum capacity” – I am totally in the dark what you mean with the equivalent system. Please explain.

Page 6, Lines 22ff: Changes in GWS also need to be discussed as link between precipitation and runoff – in drought phases, the remaining runoff is mostly sourced from groundwater. An interesting aspect not addressed here is a possible long-term trend of the GWS over the entire reporting period. I also wonder why the role of evapotranspiration in the system is practically neglected here. You have got these data, not a good idea to hide them in the Supplement. This whole paragraph (running onto Page 7) barely scratches the surface and could largely profit by more comprehensive water balance and time series analyses.

Page 7, Line 24: Again, the mysterious equivalent system of reservoirs.

Page 9, Line 1: Delete “reconnaissance”, see above.

Page 9, Line 7: “uncertainties in these estimate can be high” – Could you cite some literature or explain otherwise, why it is high and what is “high” in this context? Did you actually do the calculations for the PB and do not dare to publish the (strange?) results?

Pages 9 and 10, Section on implications for water resources: The findings presented here are rather thin. The first two paragraphs are a rug of commonplaces (e. g. “Optimal management of reservoirs to reduce impacts of future droughts requires an understanding of the controls on reservoir storage”, L. 13f) and repetitions (“Monitoring networks of GWS would be extremely beneficial, particularly because GWS can provide information to estimate baseflow to streams”, L. 7ff – “Monitoring GWS would also be beneficial for estimating baseflow to streams. . . ”, L. 19f). The remaining paragraphs are mainly a wrapup of the results presented in the preceding sections and do not really deliver new insights. Probably the entire section can be deleted without loss of relevant content.

Pages 10 and 11, Conclusions: This section should be renamed to “Summary and Conclusions”, because it rehashes again the findings before it states remarkably “This study emphasizes the importance of integrating remote sensing, modelling and monitoring data. . . ” (L. 31) – while a real integration of all the data is just what is still missing in this paper.

Figure 1: The basin maps are too small. They could be zoomed to equal size with the Brazil overview map as an inset in a corner of the elevation map (which should be coloured stepwise – the mini scale with minimum and maximum values is of no use). The colour legend of the land use map can be split and also distributed into the northern corners, there is no need to show so much off-basin area in the north.

Figure 1, Caption: What is meant by “(30 x 30 m)”? SRTM data? Then please cite the source properly.

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Figure 2: The temperature graph should be replaced by the ET graphs which deserve much more attention.

Figure 3: Should be replaced by the complete picture of the Supplement figure S17. Only three half-subjectively picked extreme years don't give an impression of the general variability.

Figure 7: This looks a bit like a student's pin board; the elements could probably be arranged more neatly in file.

From the Supplement, the following parts should be moved into the main paper: Section S2.4 on topography, climate and land use; Section S5.2 on ET with Figures S8 and S9; and most of Section S5.4 on cluster analysis with the dendrogram shown in Figure S12.

Technical corrections

Throughout the text, numbers and units appear very inconsistently sometimes in math font, sometimes in normal type; cf. 17 vs $25\,\mathrm{km^3 yr^{-1}}$ in Line 10 on Page 1. I would suggest avoiding math font in the running text as much as possible.

Page 1, Line 3: "experienced" instead of "experience".

A general mistake is made in writing ranges, e. g. 2002–2008, with a hyphen-minus, sometimes with, sometimes without spaces before and after (incorrect: 2002-2008 or 2002 - 2008). The better way to do it is with an em-dash, always without spaces, as in the first example; this can be generated by a double hyphen (`2002--2008`).

Page 1, last line: "a meteorological" instead of "meteorological".

Page 2, Line 25: "declined" instead of "declinedc".

Page 2, Line 26: Insert a paragraph break before "In this context".

Page 2, last line: "drought impacts" instead of "droughts impacts".

Page 3, Line 7: “used” instead of “use”.

Page 3, Line 16: “larger” instead of “greater”.

Page 4, Line 14 R_{off} should be italicised to R_{off} (like P_{obs} and P_{Sat} above) and can also be written without index (R).

Page 4, Line 17: “the MOD16” instead of “MOD16”.

Page 4, Line 27: There should be a minus sign in front of the number one, not a hyphen-minus. A real minus can be generated with `\textminus`. Having a little space between the less-than sign and the minus would also be fine.

Page 4, Line 29: “rank-based” instead of “ranked-based”.

Page 5, Line 11: “second drought as 2014 drought” instead of “second drought, the 2014 drought”.

Page 5, Line 16: “were” instead of “was”.

Page 5, Line 17: “the PB” instead of “PB”.

Page 6, Line 10: Re-arrange: “According to the MK U test, there is strong evidence. . . ”

Page 6, Line 17: “when the SPI was at –1.3” instead of “when SPI ~ -1.3”.

Page 6, Line 18: “the SPI” instead of “SPI”.

Page 7, Line 11: Delete the sentence “Therefore, reservoir storage effects . . . ” – it’s just a repetition of the foregoing.

Page 7, Line 14: Delete “Although dam[s] are managed primarily by humans,” and the “also” in that line.

Page 8, Line 1: “UB” is an example for an acronym that is never used again and therefore superfluous.

Supplement: There are also a larger number of typos, missing articles, unnecessary

repetitions etc. which I won't list in detail. I would recommend having everything corrected by a native English speaker before re-submission.

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