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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Large-scale impacts of hydropower development on the Mompós Depression wetlands, Colombia" *by* Héctor Angarita et al.

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Note: Updated manuscript and SI-1 (Supplementary Information) are attached

Below are reviewers' comments, each immediately followed by the Author Response and notes about related Changes in the Manuscript:

General comments: This manuscript presents a large scale, integrated, modelling study in the Magdalena basin in Colombia, exploring the impact of different scenarios of hydropower development. The number of hydropower projects in the basin is slated to increase extensively, which will potentially lead to substantial impacts on flow

regimes, reduce connectivity, and affect the extensive system of fluvial wetlands in the Mompós depression. The manuscript applies the WEAP model to evaluate the impact of different scenarios of hydropower development, based on the set of potential hydropower projects proposed, under construction, or under study in the basin. The current HP projects are also considered While I find the manuscript interesting and can see that it provides a thorough insight into the impacts of the development of hydropower potential, exemplified by the case of the Magdalena basin, I struggle to find the scientific novelty in the paper. That does not mean, however, that it is not there, but does mean that the authors should be more explicit in highlighting what scientific advances they make. To my mind the novel aspect of the paper, including the proposed enhancements to WEAP to link simulation of more local scale floodplain dynamics to basin scale scenarios, is the integrated approach that can help design basin level development strategies and make visible trade-offs that need to be made in terms of the geographic distribution of developments and impacts. To this end, I would recommend the authors elaborate how the characteristics of the scenarios influence the range of impacts on wetlands and connectivity found. What is it that alleviates impacts and what is it that exacerbates impacts? How are impacts influenced by the geographic configuration of the HP projects selected in the scenario? Do these include only small projects, or rather only large projects? What can be said about the spatial spread? Is it best to spread projects selected across sub-basins, or are one-two large tributaries "sacrificed" in lieu of low impacts in other basins? This would require a more detailed analysis of the scenarios, at least the five that have been selected from the 1000 for further study. These questions are I think relevant as they can provide valuable insight on trade-offs in the exploitation of hydropower potential. In the work developed I think the authors do have data to provide answers to these questions at their fingertips, and taking the analysis that one step further, and developing conclusions based on that analysis could develop the paper from what now seems to be more a model application study, to a scientific contribution that warrants publication.

Author response: We would like to thank the reviewer for these thoughtful comments.

The comments led to some additional analysis and substantial improvements to the manuscript. In summary the main changes are: - We performed a more detailed analysis on sampling approaches to define expansion scenarios, considering both basin-level or project-level characteristics. Results are discussed in terms of the implications for policy design. - We performed a more detailed analysis of the five selected scenarios in terms of the implications of operational approaches on system hydrologic dynamics. - Discussion and conclusions were appended to highlight other scientific advances and general implications of the presented research.

All pages: The format of the references in the main text is not in line with that of HESS, please amend. Author response: HESS style added to Mendeley and selected for use. Changes in Manuscript: All citations now in HESS style.

Page 4 – Line 28: splits again at Calamar, with a part of the flow diverted westward to Cartagena through an altered channel system that serves as a navigation canal, a part flowing into a 100-km long delta, while the main river continues to its mouth at Barranquilla. Author response: Change made. Changes in Manuscript: Manuscript now reads ". . .until the Magdalena splits again at Calamar, with part of the flow diverted westward to Cartagena through an altered channel system that serves as a navigation canal and part flowing into a 100-km long delta, while the main river continues to its mouth in Barranquilla."

Page 5 – Line 4: I agree that discharge patterns are largely influenced by ICTZ. However, there are other factors that should be included in the description that influence climatic variability across the basin. This results in the bi-model character not being equally strong across the basin, with the lower basin near the Caribbean costs often suggested to be uni-modal in character. Also the role of orography is important in determining the spatial rainfall patterns, and that may be of relevance to the interpretation of the scenarios. Author response: Agreed. Paragraph was revised to mention other factors controlling the climate variability patterns in the basin. Changes in Manuscript: Added text: "The roles of topography, soil-atmosphere interactions, the Atlantic Ocean,

and the Amazon also influence temporal and spatial rainfall patterns, resulting in the bimodal character not being equally strong across the basin (Poveda et al., 2011). The lower basin near the Caribbean coastâĂŤincluding the Mompós depressionâĂŤis often suggested to be unimodal in character, and the southeastern portion of the basin (approximately below 2°N) is characterized by a distinct unimodal pattern, with a June-to-August wet season.

Page 5- Line 13: While the linkage between ENSO and climate variability in the MRB is relevant to mention, as well as other linkages such as to the PDO, I think that Figure 2 is redundant as it does not have a direct contribution to the content. Consider removing. Author response: Figure 2 illustrates the hydroclimatic variability components present in the major tributaries of the Mompós Depression. These components are mentioned several times throughout the manuscript, making this figure useful for readers who are not familiar with the Magdalena River Basin. Changes in Manuscript: None.

Page 5 – Line 30: Break the sentence as shown below to avoid the suggestion that the HP projects have as their main purpose to reduce network connectivity and produce d/s alterations! This study focused on existing and proposed medium and large hydropower projects, including reservoirs and run-of-river plants. These can reduce river network connectivity or produce downstream alteration. Author response: Change made. Changes in Manuscript: Manuscript now reads "This study focused on existing and proposed medium and large hydropower projects, including reservoirs and run-of-river plants. Such projects can reduce river network connectivity or produce downstream alteration."

Page 6 – Line 8: It would be useful to elaborate a little on the construction of the scenarios. What was the strategy for sampling from the possible 97 projects? I assume that the selection of some projects would preclude the selection of other projects, or are they all fully independent? How were the five scenarios selected? Was this at random or was some strategy employed? This should be elaborated. Author response: In the new version of the manuscript, we incorporated additional elements in the identification

of the scenarios that allow for a more rich interpretation and discussion regarding the guiding principles that determine better basin-scale outcomes. Changes in Manuscript: Section 3.1.1. was fully rewritten.

Page 7 – Line 26: I assume that the moving average operator is applied over a six month period. It may be good to add this clarification Author response: Indeed. Changes in Manuscript: Now reads: "...and MA6 a moving average operator applied over a six-month period"

Page 9 – Line 13: I have been looking at equations 6-9 and cannot quite figure these out. What are these equations based on? Are these physical water balances, or are these empirical relationhips? It seems to me that the equations are also not consistent in dimensions, particularly with respect to time. There is a capital Z and a small z – are these the same? Author response: These are empirical functions. Steps were added to clarify the dimensions. Z and z are the same. Changes in Manuscript: Clarification on basis of equations: "The model "uses empirical functions that describe evapotranspiration, surface runoff, sub-surface runoff or interflow, and deep percolation" (Yates et al., 2005a, p.491)." See new equations 8 and 9. Z's have been corrected to be z's.

Page 10 – Lines 5-10: The authors present the algorithms they have used to improve the ability of WEAP to model surface flows. I agree that a conceptual approach would appear adequate in this case, particularly given the monthly time step. However, this more conceptual model does require extensive parameterisation. A good example is the thresholds that are mentioned in equations 10 and 11. How have these been derived? Are these based on topological information, or is this a parameter that derived through calibration? If so, then how was that calibration carried out, and were comparisons against more complete hydrodynamic models done (given that these are available in the basin)? Author response: Thanks for the comment. There are two steps in setting up the modelâĂŤfirst, to identify which areas of the model will be represented by floodplains and what are the topological connections. This is done using contextual information and does not require calibration. This first step significantly reduces the

parameters to be calibrated. The second step consists of doing a monte-carlo calibration as described in section 3.2.2. No comparisons were made with other models as there aren't any accessible hydrodynamic model results that fully cover our study area. Changes in Manuscript: A second paragraph was added to section 3.2.1, Topological representation of the floodplains system, to describe the procedure used to identify the topology of the river and floodplains.

Page 11 – Line 4: I am not sure I understand what the authors mean when saying that R2 is determined between water levels and storage. Should this not be between simulated and observed water levels? Author response: There is not enough topo-bathymetric information available to make a direct comparison of modeled and observed water levels in the wetland. We still consider R2 a useful metric to characterize the skill of the model to represent the dynamic characteristics of water storage. Model outputs of water storage dynamics are very sensitive to parameterization, so R2, in addition to NSE and P-Bias at streamflow gauges, contributes to identifying more competent models. We recognize that this is an approximation. In Section 4.2.1, we discuss the implications of this in terms of the sensitivity of the model outputs. Changes in Manuscript: "in the case of wetlands, a correlation between water levels and storage was adopted due to a lack of topo-bathymetrical data, which prevented the conversion of the model state variable (storage volume) to effective water levels in wetland units. Despite this limitation, the R2 metric reflects the model's ability to capture the dynamic character of water levels in wetland areas."

Page 12 – Line 13: I am somewhat confused by the river lengths, which are reported for pre- and post dam conditions. It is noted here that the length of the river network with Strahler > 4 is 10373. However, on the same page in the results 8311 km post-dam and 11998 km pre-dam length is reported for Strahler > 4. I guess the number on this line is the total river length to the mouth, while the second is the total length of river, unobstructed by a dam from the limit of the Mompos floodplains. A clearer indication of what length is being discussed would help. Author response: CorrectâĂŤprevious

version mixed total connected river length and length used by migratory fish. Changes in Manuscript: Numbers were changed so that all reflect connectivity of migratory fish habitat, rather than total length. Additionally, the numbers have been updated to reflect the inclusion of 3 additional migratory fish species.

Page 13 – Line 6: change sentence to “sediment loads are estimated to have been reduced due to reservoir trapping of ....” Author response: Change made. Changes in Manuscript: Manuscript now reads: “However, sediment loads are estimated to have been reduced due to reservoir trapping of. . .”

Page 13 – Line 20: I would not say the points are random that are shown in figure 9. As I understand it from the figure caption these are the points that comply with the HP expansion target (the same as in the shaded area in Figure 8. Correct? Author response: Correct. Changes in Manuscript: We clarified in the Methods Section that the points are a defined subset of the randomly generated scenarios. “Each set is a randomly sampled subset of projects (colored dots in Figure 8) that avoid one or more criteriaâĂŤprojects located on Mompós Depression tributaries (order 4+) not yet affected by artificial barriers, mainstem projects. . .”

Page 13 – Line 21: Drop “however” in the parenthesis. Author response: Change made. Changes in Manuscript: Manuscript now reads “(we did not attempt to establish. . .”

Page 13 – Lines 19-34: What may also be interesting to mention is that the range of DORw for the Cauca is much larger than for the main Magdalena, for those scenarios that comply with the HP expansion objectives. This is likely due to the simple fact that the Madgalena is the larger of the two in terms of flow. It may also be due to the specific selection of projects. So while these may be equivalent to some extent, the difference in impact is quite distinct. What surprises me is that in Figure 10 the difference between Scenario B and for example Scenario A are not that distinct, despite the significant difference in degree of regulation (admittedly this may be due it

being difficult to discern the different lines in the figure). There also seems to be very little change in all scenarios on high flow conditions, despite high degree of regulation (which implies a significant amount of storage). Author response: We simplified the plots to facilitate the comparison of the relevant scenarios. Updated plots show differences between scenarios A, B and D. Small changes in high flow conditions are the result of the adopted operational regime focused on maintaining high water levels to increase head and turbine efficiency. We appended the analysis to include two alternative operational scenarios focused on regulation of extreme high flows, by reducing the operational level of reservoirs and allowing a higher buffer volume for regulating peak flows. Changes in Manuscript: Updated figures 10 and 12, and results section.

Page 14 – Line 17 – 26: As noted earlier, were any comparisons made with the extent of inundation in the Mompós depression; either from observed flood extent (such as for the 2010-2011 event), or for model simulations using e.g. more complex HD models that do exist for the basin (which may be easier to relate to the more conceptual nature of the model presented here). The use or R2 for comparing water levels reveals very good statistics; but I would argue that correlation of monthly levels in a highly seasonal river would be expected to yield good similarity in simulated and observed patterns. Is there any information on the bias of the simulated levels at these four points? Author response: There is not full topo-bathymetric data for the entire study area, so it is not possible to perform such analysis. However, the R2 metric was found to be very sensitive to the model parameterization, and in most of the calibration trials values were found not satisfactory. Changes in Manuscript: None.

Page 15 – Lines 3- 33: In the discussion on the impacts on the floodplains during high flow events it is noted that these are small, which is also reflected in the minor change to high flows. It is mentioned that there is little control over flood events due to dam safety. While I agree that this may be the case in the current situation in the basin, with a relatively low DORw, as that increases, which essentially means that the storage volume increases, that may well change significantly. To study the possible

effect the operational rules would need to be amended, as likely these have not been implemented in WEAP to consider flood control. Author response: We performed additional analysis to explore impacts of alternative operational rules focused on reducing peak flow events. We included three new sub-scenarios for the previously identified configurations of dams that followed these alternative operational rules. Changes in Manuscript: These additional sub-scenarios (A', B', and D') are now included in Figures 10 and 12, and in the discussion.

Page 16 – Lines 12 onwards: To my mind the discussion should also include reflections on the research that has been presented, the interpretation of the results and the contribution to the state of science. Also, the authors should reflect on some of the limitations of the approach presented (such as for example the assumptions made on the reservoir operating rules). Currently the discussion discusses primarily the relevance of the work in the context of the developments, and addresses topics beyond the scope of research. This comes back to my general comments, where I think the authors should try to upscale their finding to what these may mean to the wider (scientific) community. What are the new insights that the approach they propose provide. I do think that there are quite some that could be highlighted. To my mind the integrated approach offers handles to make strategic choices on the configuration of HP development at the basin scale. Based on an that improved discussion, the conclusions could be revised as appropriate. Author response: Thanks for the comment. We reorganized the discussion section to highlight in more depth the contribution to the state of science and the broader implications of the integrated approach. Changes in Manuscript: Added Section: 5.1. Contributions of this research into the discussion chapter. Modified the conclusions section.

Page 20-24: The authors refer extensively to "grey" literature sources such as project reports etc. Please make sure that relevant details are included. An example is ESEE (1979), where only the title is included. There are several more. Please revise. Author response: Agreed Changes in Manuscript: We updated the reference style to include

relevant details (such as publishing institution and weblink where available).

Figure 12: The yellow colours are difficult to read. I also struggle to understand what the small white marks indicate, if anything. Author response: We increased the contrast in the pallete, and eliminated the white marks Changes in Manuscript: Updated figure.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-544/hess-2017-544-AC2-supplement.pdf

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