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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.

Anonymous Referee #2

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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 poten-

tial, 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.

Overall the written English is good, but it tends to deteriorate towards the end of the manuscript. Particularly the discussion seems to have been written in a bit of a hurry. There are sufficient native English speakers in the author list to fix this.

Detailed comments: All pages: The format of the references in the main text is not in line with that of HESS, please amend.

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.

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.

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.

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.

Page 6 – Line 8: It would be useful to elaborate a little on the construction of the scenarios. What as 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.

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

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?

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)?

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?

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.

Page 13 – Line 6: change sentence to “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?

Page 13 – Line 21: Drop “however” in the parenthesis.

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).

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?

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.

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.

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.

Figure 12: The yellow colours are difficult to read. I also struggle to understand what he small white marks indicate, if anything.

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