Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-162-RC2, 2018
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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Flood risk related to a fluvial system modified by dams with emphasis on morphodynamic and hydrological aspects" *by* Karina Vanesa Echevarria et al.

Anonymous Referee #2

Received and published: 25 September 2018

I started by reading the manuscript to forge my own unbiased opinion. I then read the comments by reviewer 1. I agree with almost all the remarks and the criticisms by reviewer 1. I only report here below some additional comments.

For what I understand, there is a problem with the aim of the study (as it is suggested by the title of the paper) and the real focus of the study. According to the title, I was expecting a comprehensive assessment of the modifications caused by the dam construction, including a *quantitative* description of the changes in the downstream hydrological regime (e.g., Cheng et al., 2018), of the changes of flood risk (e.g., Viero et al., 2018), and of the morphodynamic processes that can be ascribed to the dam (e.g.,

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Ronco et al., 2010). In the manuscript, I did not found any quantitative description of the changes in hydrological regime caused by the dam, nor a clear evaluation of the changes in flood risk between pre- and post-dam scenarios. That the fluvial system was modified by dams is quite irrelevant for the study, as only the current flood risk is actually evaluated. Indeed, the study is essentially an expert-driven, merely qualitatively risk assessment in the 10-km^2 areas adjacent the De Los Sauces River in Villa Dolores (Cordoba, Argentina). It is a simple case study. I do not see, from the paper, which are the repercussions of this study for the broad audience of NHESS, nor the novel methodological aspects from a scientific point of view.

In their intentions, the Authors followed the suggestion by Baker (1994), who stated that "Geomorphological flood studies, including recent advances in paleoflood hydrology, are needed as a complement to conventional hydrological approaches". Unfortunately, the conventional *quantitative* hydrological approach is substantially missing in this study. I do not see an adequate assessment of hazard, in particular from the hydrological/hydraulic point of view. For example, susceptibility is only determined based on the terrain elevation referred to the active channel (by the way, what part of the active channel is taken as a datum? The channel bottom? The banks of the main channel?). Such a procedure is in general not valid, as it does not consider the geometrical variability in a real stream. Because of different cross-sections, the presence of hydraulic works, etc., the water surface can experience substantially different rising rates at different locations for the same increase of the discharge. This means that terrain elevation alone (e.g., without the aid of hydraulic models) is not a sufficient information to determine the flood hazard of areas adjacent to the watercourse. Furthermore, the geomorphological analysis is not functional in order to determine the residual risk in the post-dam scenario, which mainly comes from sub-basins located downstream of the dam.

The points above entail that the main conclusions of the study are not supported by the analysis carried out by the Authors. Indeed, the increase in flood risk in Villa Do-

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lores city and peripheral localities, for low to moderate magnitude floods, is not due to the construction of the dam; rather, the urbanization of floodplain areas increased the exposure/vulnerability that, in turn, led to increased flood risk. The dam, by significantly reducing flood discharges, might have induced (incautious) people to urbanize exposed areas, in a process that is similar to the so-called “levee-effect” (see, for example, Hutton et al., 2017, and references therein).

In addition, consider that geomorphology is a discipline that studies the processes that shape the land. In many parts of the paper, terms as “geomorphology” and “morphodynamic” are improperly used to indicate nothing more than the topography of the study area. For example, susceptibility is only determined based on the terrain elevation referred to the active channel. The identification of geomorphological units is merely descriptive for this study, and does not add substantial information for flood risk evaluation.

Finally, consider that the quality of the English is unacceptably low for NHESS. Please consider revising especially the grammar and the syntax, possibly with the support of a native speaker. Furthermore, the Authors should be more rigorous and careful in using technical terms.

Specific points

-The information content of the Abstract is very poor.
-Please consider to limit the references to grey literature (conferences, thesis), especially in Spanish. This particularly applies to the general part of the Introduction.
-l.11: Please revise the first sentence
-l.70: what is “energy” in this context?
-l.116-121: information here is insufficient. Please demonstrate that the hypothesis of uniform flow is verified at locations used to estimate the discharge. In general, discharge can be estimated with greater accuracy in control sections (if any), i.e., when subcritical to supercritical transition occurs (for example at bottom sills, narrow-

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ing cross-sections, etc.).
-l.143: a slope cannot be given in meters.
-l.215-ff.: I do not see any hydrological analysis that supports the attribution of return periods to discharge. (By the way, which scenario is to be chosen if the discharge is between 80 and 100 m^3/s?)
-l.239-ff.: how is the hazard obtained from susceptibility and threat?

References

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