

Interactive comment on “Multi-method assessment of reservoir effects on hydrological droughts in an arid region” by S. Rangecroft et al.

Anonymous Referee #2

Received and published: 19 December 2016

The manuscript aims to assess the effects dam building on hydrological droughts in a case study of the Santa Juana dam in Chile. This is done by establishing liner relationships (% change) between upstream and downstream sites of the dam based on observed and modelled data (with natural and human influenced scenarios).

In its current state, I cannot recommend the publication of the manuscript, as the methods applied and the analysis conducted is not clear and appears to lack methodological rigor and the interdisciplinary aspect and dynamic interactions (here the interactions between humans (dam building activity) and the hydrological system (with regard to drought characteristics)) are not well modelled/analysed (as specified in the comments below).

General comments:

Printer-friendly version

Discussion paper



Overall I'm not sure if the article fits the interdisciplinary scope of the journal which focuses on the interaction in the earth system. As it is written currently the manuscript would better fit a journal focused on hydrology. This is of particular concern as at several instances in the manuscript a solid background knowledge in drought hydrology is required to understand the statements, which cannot be expected from the interdisciplinary audience of this journal (see also some of the specific comments below). I therefore encourage the authors to focus more on the journal and its audience when revising the manuscript.

Additionally, I would encourage the authors to better streamline the research presented, as the train of thought is not clear. Currently, throughout the manuscript several new aspects are being invoked in the middle and the end of the paper that should have been already considered and presented earlier on (e.g. sensitivity analysis).

Apart from these aspects, my main concern is the use of that parts of the analysis are performed on the output (streamflow) of model that is supposed simulate human influence in the catchment, although the model under the 'human influence' scenario is not simulating the river discharge correctly (L 439). Which calls this part of the research into question. Additionally, the authors do not provide any information on the WEAP model setup (e.g. reservoir operation rules, water abstractions, etc.) and sensitivities to initial values/parameters, which makes it impossible to reproduce of the research and draw any conclusions on the possible influence of the model setup on the results obtained.

To assess the influences of human on droughts a detailed analysis of the reservoir management rules and water allocations is needed, as it is not just the presence of the built structure itself that has an effect on drought but predominately how the water is managed. This very important aspect is hidden in the WAEP model and missing from the analysis and not well covered in the discussion.

In addition, the 'upstream-downstream approach' presented in the method (i.e. the

[Printer-friendly version](#)[Discussion paper](#)

direct comparison of changes in selected drought indices upstream and downstream of the dam) is indicated as a 'new method for quantifying change' (L 144). I agree with the authors that such a 'upstream-downstream' comparison is useful in trying to determine the influence of a dam/reservoir, however, I think this does not qualify for the label 'new method'.

Additionally, I have some concerns how the comparison is being implemented in this case study. The additional catchment area between two hydrological stations that are being compared is 500 km² (which is about 1/3 of the entire downstream catchment). With such a large additional catchment area contributing to the 'downstream part', the drought characteristics that can be evaluated at the two stations might already be naturally altered by the hydrological processes occurring (as opposed to measurements that are directly taken upstream and downstream of the structure). The presence of the large additional contribution areas makes it difficult to compare the changes in a (temporally) lumped setting, as there is a lot of scope for non-linear hydrological processes occurring. This issue needs particular attention, as the pre- and post-dam periods have different length and therefore might have experienced very different hydrological settings. Without a thorough assessment of the temporal stability of the differences between upstream and downstream through for example a sensitivity analysis of % change downstream for different temporal windows (during the pre-dam period) verifying temporal stability the current methodology is not very meaningful.

Finally, the paper is missing a detailed discussion section. In the individual paragraphs some discussion is provided, however for clarity I suggest moving these scattered parts into one single coherent discussion section.

Specific comments:

Title: Please be more specific and either replace 'in an arid region' with the study region or add the study area to the title after a colon.

L2: Please clarify why 'increased pressure on water resources' 'lead to INCREASED

Printer-friendly version

Discussion paper



management'. Consider rephrasing.

L13-15: Many of the 'findings' described in the abstract seem to be obvious. E.g. a delay in the timing of the drought with the presence of the dam. Please make sure to describe the findings more concrete.

L52: 'human activities . . . can positively affect the hydrological system'. I think I know what the authors try to convey but I'm not sure if 'the histological system' is the right wording. Please rephrase.

L 53: Please define what 'resilience' means in this setting. This is would also be beneficial in L65.

L 70-72; 'population changes'. Please be more specific what these changes entail. This also applies to 'changes in supply' and 'alterations in precipitation patterns'.

L74: replace 'impacting' with 'affecting'

L 75: 'vulnerable areas' in what sense? Please elaborate.

L 76: Please elaborate WHY 'it is currently unclear on what is the best method' (what are the difficulties, why has this not been resolved so far) instead of just stating that it is unclear.

L 77: 'these research gaps' . Please elaborate which ones.

L 80: to avoid confusion I suggest stating that the dam was 'operating in 1998' and not 'built by 1998'.

L99: Figure 1: The labels in the elevation map is not readable as it is too small and blurred.

L 105 'the precipitation is inter-annually variable'. Please consider re-wording. This also applies to L 109 'most vulnerable'.

L 116: To make it easier for the reader to interpret this plot I suggest adding either

Printer-friendly version

Discussion paper



labels to the Figure marking the seasons or adding the details seasons to the figure caption. Additionally, please specify which variable is shown with the lines and the bars.

L 124: Please change the word order to 'the main water regulating structure'.

L 131: Please elaborate how the 'partial failure' and failure thresholds were established/do entail.

L 133: Before final submission, please make sure that quality of the figure is higher, as currently the labels are blurred

L 142: replace 'about' with 'of'.

L 159-160: please do not just state that missing data was interpolated and replaced with zeros together with the reference, but also elaborate for the interdisciplinary readership why this can be done in this setting and how this might/might not influence the results of the study.

L 168: add space between 'and'

L 181 'This method is based on the observation-modelling framework. . .' Please briefly elaborate what this framework entails.

L 186: replace 'integrating' with 'integrated'.

L 186-194: A detailed account of the WEAP model setup is needed. Please elaborate with at least one additional full paragraph.

L 190: Please reword, an objective functions does not assess the 'accuracy'. Additionally, please give the formula that was used to calculate the NSE, as log values are mentioned and the original Nash Sutcliffe efficiency (NSE) does referenced does not allow for this. Please elaborate for the interdisciplinary readership why log values were used. Additionally, I strongly recommend not only relying on the NSE (which is not very sensitive to systematic model over- or underprediction, which could have a strong

[Printer-friendly version](#)[Discussion paper](#)

influence on the drought indices derived), but also using additional model evaluation measures to quantify absolute or relative volume errors. See also Krause et al 2005. Please also elaborate why monthly data was used to assess the model performance.

Section 3.3.1. & 3.3.2: The first parts of the sections are generally written in the style of a literature review stating recommendations based on previous studies. However, often it remains unclear WHY other studies recommend a certain choice of threshold or index. I would recommend cutting these sections and focusing only on the parts that are relevant for this study and explaining why certain choice were made. Equations 1-3: All equations rely on the assumption of temporal stability (see also general comment above). Please add assessment.

L 303 - 308 the name given ('Expnum') to the variable that is supposed to represent the 'expected 'natural' value' is confusing. To avoid confusion I suggest naming the variable 'Expnat'.

L 308 & L 317: please use different symbols/names to distinguish the different ways how the '% of human influence' was calculated. Additionally, please make clear in the text what the difference between the two equations is.

L 330: Please specify/quantify the result instead of just stating that 'drought events APPEARED to be reduced'

L 331: '...similar meteorological droughts occurred...' which SPI index are you referring to? Please specify.

L 341: I think 'maximum duration' should read 'number of drought events' with the associated correction of the following numbers? As this is the only variable in the table that increased with the in the calculation of the % of human influence.

L 355: Do you mean 'discrepancies' instead of 'discretions'?

L 362: 'blocky pattern' does refer to which SPI? SPI-12? Please indicate in text.

[Printer-friendly version](#)[Discussion paper](#)

L 362-365: I'm not sure why this is presented here in the results section. This should either be taken into account beforehand or moved into the discussion section.

L372: Figure 6: Panel a) is blurred. Please provide a higher resolution. Additionally, please add note to legend that the high values on the y-axis are cut.

L 381: why is it here now the normal NSE and not the log value as presented on the method section? This is confusing.

L 381-384: Please provide a Figure showing the observed and the simulated discharge to complement the NSE statistics before analysing the series for droughts.

L 401: This is not visible from Figure 7. Please provide some way how the reader follow this reasoning (e.g. summary statistics on the seasonal distribution of droughts)

L 410: the modifications in the hydrological regime are not just cause by the presence of a dam/reservoir but also strongly depend on the management/operation rules! Please also add this to the discussions

L 418: Discharge is given in mm/months in Figure 7. If this measure is used (as compared to m³/s) additional information is needed to be able to know the flow volume. Please add. (This also applies to Table 1- 3 where discharge is given in mm/d) Additionally, I'm not sure if I understand this correct but I think Figure 7 shows the 'downstream site'. If this is the case, please make clear that the reader understands this. If not, please make sure that it is clear what the figure is showing.

L 465-466: Why not compare SI and TL directly? I think this is an important part to understand which method is most appropriate for analysing the human influence on drought. I therefore suggest adding a section in this.

Section 4.6 (529-573): I'm not sure if this section detailing the effect of the dam on specific drought events is appropriate as the WEAP model performance is poor. Without any details on the models setup any results presented could be artifacts of the rules established in the WEAP model.

Printer-friendly version

Discussion paper



References: Although I recognise that one of the authors has published several papers on droughts I think that the self-citations are too prominent in the text, particularly in the introduction section (8 self-cites were the authors is among the first 2 authors) but also in the rest of the paper. I would encourage the authors to strive for a more balanced review of the literature/methods particularly with regard to publications outside of the authors network.

Tables: Please format the tables according to the publisher guidelines: 'Horizontal lines should normally only appear above and below the table, and as a separator between the head and the main body of the table. Vertical lines and shading must be avoided'. Additionally, please check with the typesetting if the symbols used in Table 5-8 can be used in the typesetting process.

Reference used in review: Krause, P., Boyle, D. & Bäse, F. Comparison of different efficiency criteria for hydrological model assessment. *Advances in Geosciences* 5, 89–97 (2005).

[Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2016-57, 2016.](#)

[Printer-friendly version](#)

[Discussion paper](#)

