

We thank the reviewer for the time and effort with the review. We have addressed every point that they have made, with our responses below in blue italics.

Anonymous Referee #2

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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:

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.

We hope that you find that we have addressed a lot of your comments, mainly through a refocusing and restructuring of the paper (please see new proposed title and headings and sub-heading).

“Evaluating observation-based methods to quantify human influences on hydrological droughts”

1 Introduction

2 Methods

2.1 Observation data

2.2 Drought analysis approaches

2.2.1 Threshold level

2.2.2 Standardised Indices

2.3 Sensitivity analysis

2.4 Estimation of the human impact on drought characteristics

2.4.1 Percentage change due to human influence in observation data

3 Results

3.1 Results of human influence on drought characteristics downstream

3.2 Sensitivity analysis of drought analysis methods (SI vs TL)

4 Discussion

4.1 ST vs TL

3.2 Comparison between observed data and modelled data

4.3 Quantifying human influence on droughts: ways forward

4 Conclusions

We deem the subject of this paper, the anthropogenic influence on hydrology and droughts, to be an important interdisciplinary geoscience problem, making it appropriate for Earth System Dynamics. The research topic fits into the broader theme of “interactions between human and “natural” processes in the Earth System”.

We will change some of the terminology and writing to be more appropriate to an interdisciplinary audience.

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).

We propose to rewrite a number of parts to the paper and restructure it so that the focus is clearer, including the title, the abstract and the results (please see revised headings and sub-headings to demonstrate this).

We also agree that the sensitivity analysis could be moved forward in the paper to hold more importance to the results and the paper itself and the results section now focuses on 1) reporting briefly the observation results using the SI and TL methods; 2) comparison between the two methods – the 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.

We agree that there is more information needed about the WEAP model set up itself, a comment also made by reviewer 1. However, we have completely removed the WEAP aspect of the paper.

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.

We would like to stress that reservoir management is not the focus of the study. We completely agree that the reservoir management rules and water allocations are needed for a more detailed analysis; however, what we can provide here with the data is an initial assessment and quantification of how the presence of that reservoir is influencing hydrological droughts downstream compared to those observed upstream in the same time period.

Using observations from the pre-dam period, we can also estimate how much the reservoir has changed droughts from what could be expected at the downstream station based on upstream observations. By using observation data the management of the reservoir is included by comparing the upstream and downstream stations of the reservoir.

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

In the paper we make a new application of this method, to quantifying the human influence for hydrological droughts. We will look to change the label ‘new method’ to ‘new application’ of this method for droughts.

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.

We agree that our approach does not take into account a possible non-linear relationship between the upstream and downstream site, however it is a preliminary assessment of the propagation, and is more accurate than not accounting for it. We do also present the results of the percentage change downstream if readers do not agree with our method.

Furthermore, this proposed method is better than when pre-dam time periods are compared directly with post-dam time periods which have completely different driving external forcing of meteorological conditions.

However we will add this limitation in the paper: that we are using the assumption of a linear relationship between upstream and downstream stations to estimate the human influence.

It is true that the pre-dam and post-dam periods have different record lengths, and it is for this reason that we standardised the number of droughts to be per decade rather than the absolute number (record length influences the value), but record length does not affect the values reported for duration and deficit.

We conducting the extra analysis suggested by the reviewer (different temporal windows during the pre-dam period), however this is a difficult task based on the fact that droughts span over multiple years, and a slight delay between droughts can then make them disassociated when occurring in two adjacent time windows.

Instead, we directly compared a number of drought events in the pre- and post-dam period (table below). We can verify that the changes observed in individual drought events during the post-dam period are much larger than those observed in the pre-dam period, implying that the changes measured are outside of the range of noise and uncertainty. Most of the numbers of the pre-dam period are in the same range, which gives us confidence in using this approach. Although we think it is better to do this analysis on averages than on individual events, the fact that the post-dam numbers are substantially higher indicates that the effect of the dam is overruling the propagation effect.

	Sample drought event	Start date upstream	Start date downstream	% change downstream	
				Duration	Deficit
Pre-dam	Drought 1	08/1968	08/1968	+1.5%	+28%
	Drought 2	08/1975	09/1975	-18%	-30%
	Drought 3	08/1979	11/1979	-26%	-10%
	Drought 4	11/1990	11/1990	-5%	-35%
Post-dam	Drought 5	09/2009	05/2010	-79%	-90%
	Drought 6	09/2012	01/2013	-81%	-98%

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.

We agree that the paper as it stands does not have a standalone discussion section. We had decided to weave our discussion points in with the results section given that number of different aspects that are presented in the results section.

However, we have now restructured the paper to focus on the results and then the discussion separately.

We thank the reviewer for their specific comments, and we have changed them or addressed all individually. Some of these may become irrelevant with the new restructuring of the paper.

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.

New title: "Evaluating observation-based methods to quantify human influences on hydrological droughts"

L2: Please clarify why 'increased pressure on water resources' 'lead to INCREASED management'. Consider rephrasing.

We have edited the sentence to now read "Human activities affecting hydrology are increasing in occurrence with growing pressure on water resources and availability, however, the impacts of these anthropogenic activities on hydrological droughts have yet to be incorporated and assessed."

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.

The delay in timing of droughts with the presence of the dam are not stated so much in the literature, and our results are the quantitative assessment of the changes.

However, we have re-written our abstract and the focus is now removed from the reservoir results, therefore this comment should not be an issue anymore.

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.

Thank you for picking this up, we have changed this sentence to say "water availability" instead of "the hydrological system".

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

We define the term resilience as "the ability of a system to persist in a given state subject to perturbations" (Folke et al., 2010) to represent the ability of the community to cope with changes. However, we have rewritten this section and the term resilience is not present anymore.

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'.

"Population changes" includes population increase and westernisation of lifestyles. "Changes in supply" is already explained by the rest of the sentence implying that changes in temperature and precipitation will affect water availability. Alterations in precipitation patterns refers to the fact that not only is precipitation predicted to decrease and increase with climate change, but the timing and intensity of precipitation is expected to change.

L74: replace 'impacting' with 'affecting'

Changed.

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

"Vulnerable areas" is used here because we have already stated what factors are affecting these regions in regards to their water availability (increase in demand and changes to supply) and that they have low resilience. However, once again in the rewriting, we have removed this sentence.

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.

We have addressed this in the revised version.

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

The research gaps are identified in the two sentences prior: "Therefore, there is a need to improve our knowledge on how human activities are impacting on drought to enable better drought preparation and mitigation, especially in these vulnerable, arid regions. It is currently unclear what is the best method for assessing and quantifying the impact of human activities on hydrological droughts."

We have re-written this section and we have specifically highlighted the research gaps before mentioning how the paper aims to address them.

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

Changed.

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

Changed.

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

"the precipitation is inter-annually variable" has been changed to "the precipitation can vary from year to year with the ENSO".

"Most vulnerable" has been changed to "most susceptible" .

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

We agree and have changed the caption to read: "Figure 2: Seasonality plots for monthly precipitation bar charts and overlaid discharge line graph using daily data (1965-2013)." And we can add on vertical dashed lines framing the Chilean winter (May and August) and stating "Winter" on that space on the graph.

This figure has also been removed from the main manuscript and is in the supplementary material instead.

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

Changed.

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

Changed to: "During the recent multi-year drought (2007-2015), by 2011 reservoir levels dropped below levels of "partial failure" (<100 Mm³, Figure 3), levels set by the Huasco River Supervisory Board, resulting in them implementing severe water restrictions (through its operational model)."

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

Changed.

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

Changed.

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.

Changed.

L 168: add space between ')and'

Changed

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

We can easily add a sentence or two introducing this framework. This is now in the introduction when the research gaps are highlighted: we include more about existing methods such as the observation-modelling framework and how this paper tests a more observation based approach, the upstream-downstream approach.

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

Changed.

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

We have completely removed the WEAP modelling aspect so this should not be a problem now.

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 under-prediction, which could have a strong 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.

The NSE values were indeed logged, however now that the WEAP model has been removed; this is no longer an issue.

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 choices were made. Equations 1- 3: All equations rely on the assumption of temporal stability (see also general comment above). Please add assessment.

Whilst we agree that these sections read as literature reviews, we think that it is important to have this background information in for the interdisciplinary audience, so that information is not lost when showing the results from the two different methods. These sections now hold more value as the focus of the paper is on comparing the two methods.

Equations 1-3 do not require temporal stability because we are directly comparing the drought events of two situations which look to use the same time period and the same inputs (e.g. upstream and downstream).

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

We agree that these abbreviations are confusing and we have indeed re-named: "This generates an expected "natural" value (Exp-down) for the post-dam period which could be directly compared to the actual Qobs-down post-dam value. The difference between this expected value (Exp-down) and the actual observed value (Obs-hum) gave an overall percentage of human influence in the post-dam period (Eq 2)."

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.

We have changed the name of equation 2: Percentage of human influence (observations) (%)

Equation 3 has been removed as it was linked to the modelling data.

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

We have re-written so that the quantified results are outlined briefly as the results sub-section from the data itself.

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

We can change this to be specific to the SPI we are talking about, SPI -6.

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.

It is correct at the moment as maximum duration, please see table 2 post-dam for the values that this sentence is referring to.

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

Changed.

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

SPI-6 has been specified in the text now.

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.

The paper we submitted had a combined results and discussion section, which is why this is presented here. However, we have restructured the paper and so we have moved this to later on, in a 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.

Changed.

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

We did miss out the key word 'log value' here and in fact we do refer to only the log NSE values throughout, however this is now not in the rewritten version.

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.

In the version of the manuscript we submitted, we did already show these discharges in Figure 6 (observation discharge) and Figure 7 (simulated discharge). However, now that the modelled data has been removed, this should not be an issue anymore.

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

This would be a very good addition to the paper and we will indeed add in a summary statistics table 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

This is completely correct, and different reservoir management and operation rules would lead to a different impact on droughts downstream. For this reason, we have now stressed the purpose of reservoir at the start of the paper in the introduction section, and we can add a statement into the discussion section about this.

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.

We can indeed add the m3/s discharge values as well as the mm/d or mm/month if needed. However, the catchment areas are given in Table 1 and by using mm/d or mm/month, the values are more comparable. Table 1 has actually been removed in the rewrite as the data is described in the start of the methods and the table is not needed.

You are correct about Figure 7 showing only the downstream station and this has now been clarified in the figure caption, however this figure is not in the revised version that we have been working on.

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.

We do compare SI and TL methods, showing the results from both and the sensitivity analysis. They are fundamentally different methods, and therefore differences are seen between them, because one involves using the whole period to establish its threshold (SI) and the other uses a reference period to help remove the human influenced data from the threshold (TL). All of these differences are explained in Section 4.4 and section 4.5 of the submitted manuscript, and these remain in the rewritten version, with more emphasis on the comparison.

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.

We agree that the use of the WEAP model is not a primary focus here because of its lower performance NSE log value. However, it does show some interesting suggestions about the 1969 drought event which could not be established by the observation data: the idea that the presence of the Santa Juana reservoir will have not been able to provide much buffer against the Great Drought of 1969. We have removed the aspect of the WEAP model, and now we only refer to it to demonstrate the advantages of using modelling data, without giving concrete results because of the NSE values.

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.

We look back at the paper and agree that there is a need for a more balanced reporting of the literature, and we have addressed this and will hope that the revised version would be found to be more balanced.

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.

Changed.

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).