

Responses to Reviewer 1

We are very grateful with the comments provided by the anonymous Reviewer 1. It is clear that her/his comments come from a deep analysis of the work we present here. Below you will find our response to each.

Is the Río Imperial streamflow variability representative of the entire SCC? I think it is not. Please provide evidences about this point, especially for summer. It is an important issue due the title of this paper include a large region from 35°S to 42°S in Chile.

RESPONSE: Thanks to your observation we reassessed the sentences including the 35-42 strip and decided to modify the manuscript to “SCC ~37°-42°S”. Previous studies (Rubio-Álvarez & McPhee, 2010; Muñoz et al., 2016; Lara et al, 2008) found that the Río Imperial fits better as representing the region south of 37.5, as for example a recent reconstruction of the BioBío river (~37.5°S) was similar to another built for the Puelo river (~42°S). However, we consider important to indicate that available observational datasets for the period 1980-2010 suggest that the region south of 35-42°S is a hydroclimatic cluster (see González-Reyes et al 2017), especially during the summer, our target season. The figure R1.A shows that our reconstructed streamflow (dark blue) correlates significantly (as described by the p-value) with observed streamflow of each individual station, and also each individual station correlates significantly with the composite time-series produced in our study. We will include this new analysis in the paper in order to provide further evidence on the relevance of the Río Imperial reconstruction for this region.

How an accurate calculation of natural streamflow variability can help to anticipate possible consequences in the water management? This is the justification or motivation of the analysis. My point is the following. If you proved that streamflow variance (or extremes) in the past was larger than the expected by models for the future, how this information is useful for water management? Perhaps, if droughts were more severe in the past, without a major extinction or decrease of vegetation, you can ask why we expect a major problem in the future? The major uncertainty is related to in any case with the water demand, but not with the natural or anthropogenic origin of the droughts. It is the minimum ecological discharge the variable benefited by this study? If so, I guess the water management issue is restricted to the decisions depending on this variable. Can you focus on this point?

RESPONSE: We could not agree more with this assessment; the major uncertainty is related to water demand, which we don't analyze in detail here. However, we want to clarify that the main result in our work is the increasing frequency of dry summers, which is unseen in the analyzed period. In our view, this implies a new hydroclimatic scenario that might make the region more sensitive to changes in water demand, such as a possible utilization of all water rights in the watershed. In the abstract, results and discussion section we have modified the text in order to emphasize these findings. On the other hand, that our work finds some summers comparatively drier than what is seen in the available observations show, suggests that calculations of return periods can benefit from research like ours in order to produce fine-tuned statistics, specially in the determination of confidence intervals for the minimum ecological discharge. This is a statement we already have in our paper but it may be clear if we divide the section of discussion and conclusion in two, as the reviewer number 2 suggests.

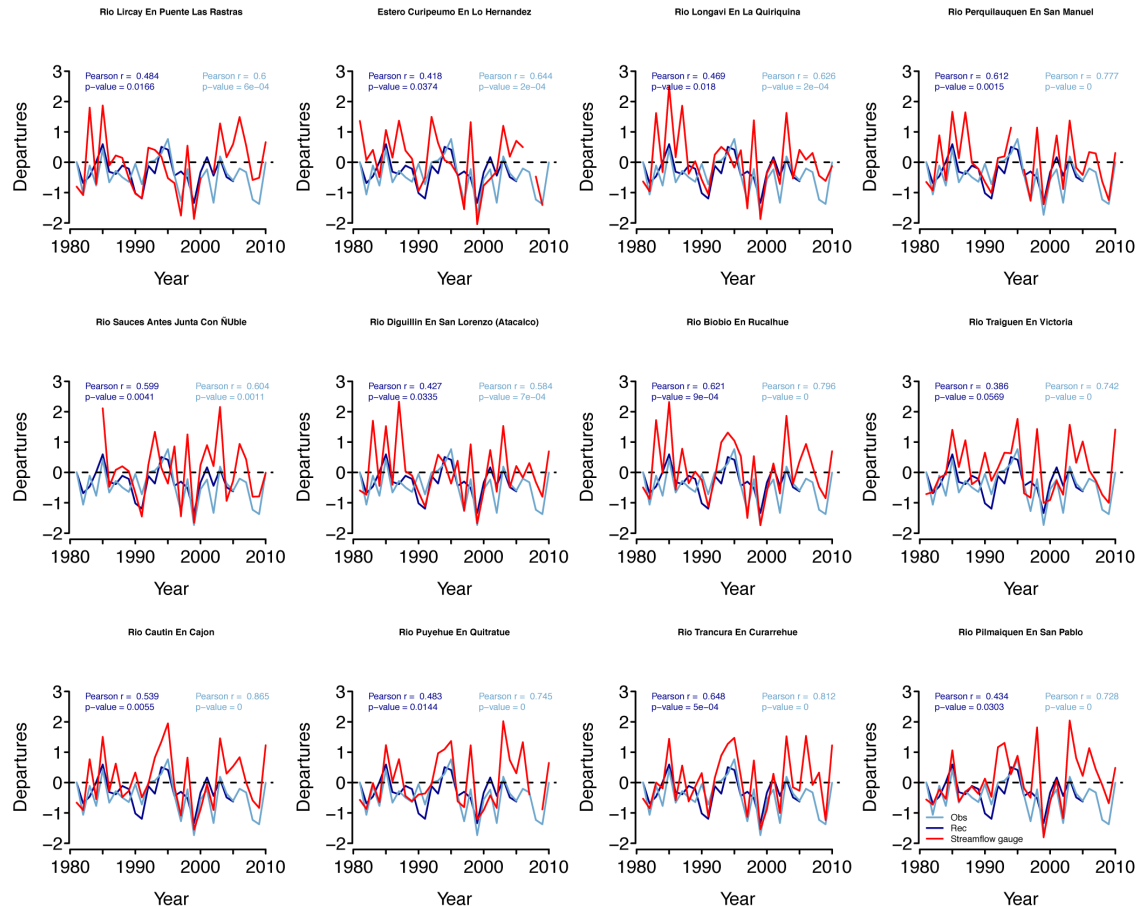


FIGURE R1.A: Comparison of available streamflow data (red), our tree-ring reconstruction (dark blue), and the composite streamflow for the period 1980-2010 (cyan).

It would have been very instructive if you were to count with data until 2014 or 2015 with the aim of calculating the return period of droughts or the recurrence rate of drought events of the mega-drought mentioned in the work of Garreaud (2015). According to this author, the mentioned mega-drought 2010-14 is the largest on record. Can these statistics used in your work show the extreme nature of the mega-drought? (at least for the instrumental record).

RESPONSE: We thank you very much for your suggestion. We have decided that comparing our results with the recent developments as depicted in Garreaud et al (2015) is important and necessary. We extended our reconstruction including the period 2011-2015 (“megadrought”) and found that, despite being an unprecedented dry period for the instrumental record, it does not rank as the highest in the reconstruction (see Table R1.A below). We think this finding is very interesting because a recent paper studying the megadrought on winter-spring precipitation found that the period 2011-2015 as highly unusual in the last 1000 years (Garreaud et al 2017). We also find this period as the driest of the streamflow instrumental record, but it is not too different from the period 1996-2000. In addition, in the reconstruction the 2011-2015 period ranks fifth but with an anomaly that is less than a half of the driest (1897-1901). The Figure R1.B shows that the summer reconstruction for 2011-2015 is far from the most extreme, but corroborates our main finding that dry years become more recurrent since 1980. We will update our results and discussion with these findings.

Table R1.A: Updated 5-year rankings for the instrumental and reconstructed periods

Reconstructed period (1709-2015)	Low flow		High flow	
	5-yrs	reco	5-yrs	reco
	1897-1901	-1.235	1951-1955	1.087
	1753-1757	-0.764	1829-1833	1.016
	1811-1815	-0.737	1934-1938	0.842
	1996-2000	-0.586	1834-1838	0.816
	1987-1991	-0.584	1797-1801	0.751
	2011-2015	-0.556	1939-1943	0.749

Instrumental period (1947-2015)				
	5-yrs	reco	5-yrs	reco
	2011-2015	-0.778	1955-1959	0.972
	1996-2000	-0.777	1950-1954	0.841
	2005-2009	-0.681	1965-1969	0.727
	2008-2012	-0.677	1971-1975	0.560
	1987-1991	-0.382	1976-1980	0.271
	1982-1986	-0.378	1992-1996	0.200

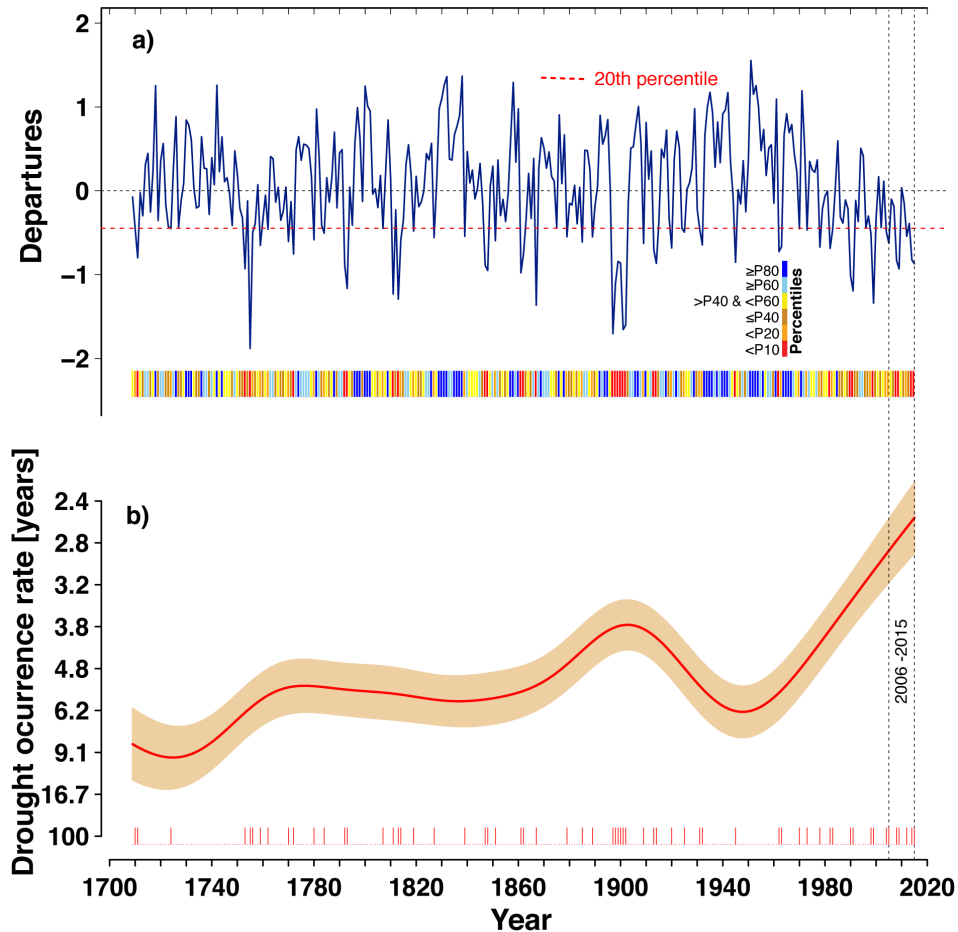


Figure R1.B: Updated calculations of departures and recurrence intervals for the period studied, including the megadrought (2011-2015).

I understand the use of the Southern Oscillation Index (SOI) instead the Niño 3.4 SST anomalies, due the longest record of atmospheric pressure data in Tahiti and Darwin. In fact, SOI is not usually used in climate studies, due its large "noise" in the intraseasonal timescale, compared to the more smoothed evolution of the SST index in the

central equatorial Pacific. Anyway, I expected the used of the SOI directly calculated from stations but you used the NCEP-NCAR reanalysis. What is the justification then?

RESPONSE: Thank you very much for catching this up. We will re-write these sentences since they don't make it clear we do use a SOI index built from observational data. We utilized the SOI index that begins in 1866 and is downloaded from the following link: <http://www.cgd.ucar.edu/cas/catalog/climind/soi.html>.

About equation (1) and Fig. 3. I am not dendronologist, so it is surprising the low covariance shared between these reconstructions, even when the samples are larger. The trees are not responding in the same way to the atmospheric forcing? (water availability for instance). This explain equation (1)... but still I need an explanation in the text for this behaviour. Looking at figure 4 (upper-left panel) the reconstruction looks good.

RESPONSE: We appreciate this comment that allows us to further clarify our analysis of individual chronologies. LYV and PAG correlate positively with same-year streamflow (0.38 and 0.43, respectively), with the streamflow of the previous year (0.53 and 0.41, respectively), and with streamflow two years before (0.3 and 0.23, respectively). Conversely, PIN shows negative correlations with previous years (-0.56 for the 1st and -0.38 for the 2nd previous year). The negative correlation for PIN is because the site is a narrow sector with recurrent fog, where atmospheric moisture and precipitation in summer may produce a relative reduction in incoming radiation and temperature; these conditions may reduce the rate of the tree-ring growth. Mundo et al (2012) already found this behavior for PIN as part of a large group of chronologies. We will include this explanation in our manuscript.

Fig. 4. It is clear from the comparison between reconstructed and observed streamflow that the reconstruction captures the low frequency but not the interannual variance, although the coherence shows a peak on 2.8 years. Based on this finding, why the authors can expect a reliable comparison with ENSO? Because it is the most important driver at interannual timescales? In fact, what is the reason for not using the Inter-decadal Pacific Oscillation instead ENSO? Why SAM? Please provide references that reinforce your thoughts about possible drivers.

Response: We fully agree with the reviewer in that the reconstruction fits well with the low frequency, but we also believe it does well with the interannual variability because there are only two peaks not being captured. Figure 1 shows no peak on these years for Quepe (QUE ~1956) and Muco (MUC, ~1982) but these features are averaged-out in the composite. Thus, although our reconstruction does not capture every fluctuation of the composite time-series, it likewise evidences correspondence with observations. Nevertheless, we applied new analyses to the data. Utilizing the Blackman-Tukey spectral method (Ghil et al 2002, Figure R1.C) we see that the Río Imperial reconstruction has high frequency cycles (2-7 years) and mid-to-low frequency (> 8 years). A Multi-taper method and a Singular Spectral Analysis reveal that a ~4-year cycle captures 10% of the variance while a ~7-year cycle captures 7%. On the other hand, a 16-20-years cycle corresponds to 20%. We also performed a Continuous Wavelet Transform Analysis and found that a 16-32-years frequency is significant between 1800-1950; high frequency cycles occur along the whole period but appear more significant after 1900.

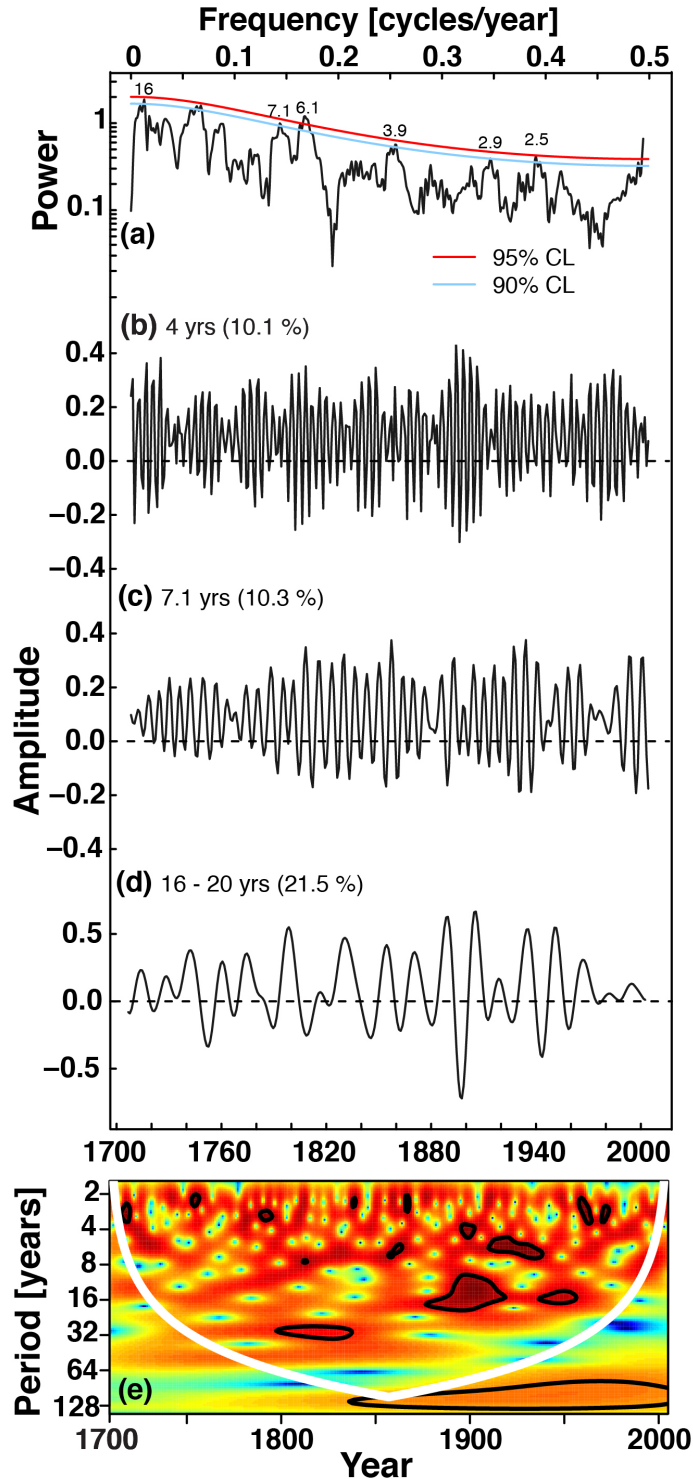


Figure R1.C: Spectral Analyses of the time-series demonstrating that it captures high frequency

Regarding ENSO, there is abundant literature correlating it with hydrological variables at the annual scale, but there is no such complete information for the summer. As we explain in our manuscript, Urrutia et al (2011) find annual discharge in the Maule river (~35°S) well correlated with ENSO, while Muñoz et al

(2016) find the BioBío river (~37°S) correlated with SAM. This was the criteria we utilized to perform these correlations. The río Imperial is south from the BioBío and our work is the first solely focused on summer dynamics. Barría et al (2017) analyzed the upper section of the BioBío dividing the year in two sections: October-March (OM) and April-September (AS). They find that PDO correlates negatively with AS runoff and positively with OM; For SAM, they find positive correlation for OM. Giving your comment and these new studies, we found no reason to not perform comparison between our time series and the IPO. We compared our time-series with the IPO reconstruction presented in Vance et al (2015) and found a statistically significant negative correlation between IPO and the 16-20-year cycle of our reconstruction (-0.38, n=295), although it looks more coherent during the 20th century (Figure R1.D). We will include these new analyses into the manuscript.

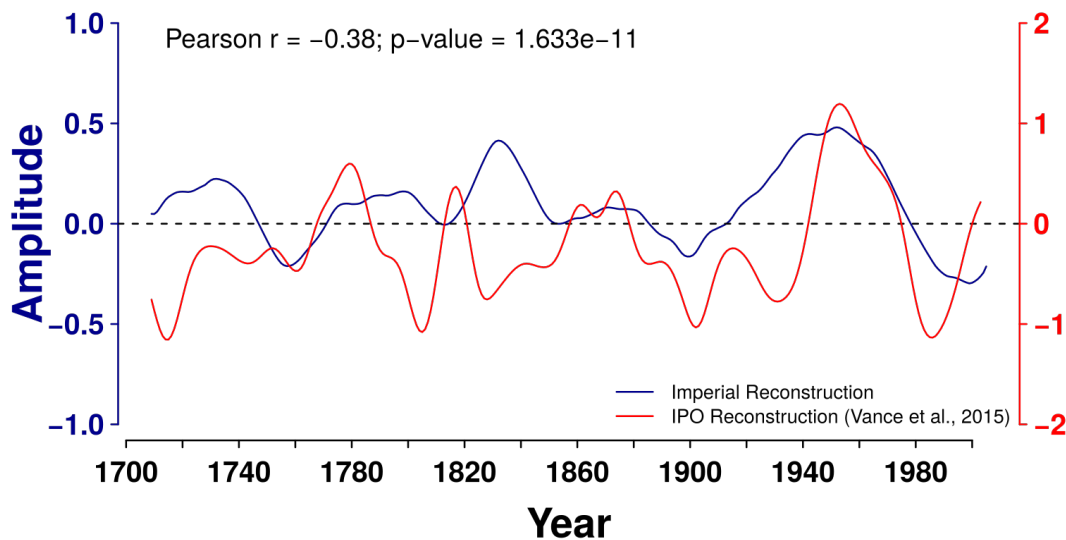


Figure R1.D: Comparison between our reconstruction and the IPO (curve inverted for readability).

Page 7, lines 25-26. The sentence is misleading, because Garreaud (2015) and Boisier et al (2016) define the mega-drought since 2010, exactly when your information stop. Have you considered the possibility of interdecadal variability? IPO changes to its positive (warm) phase at the end of 70s, changing to a negative (cold) phase ant the end of 90s. This can be seen as a negative trend since 1980... Please, provide some discussion about this possibility.

Response: We agree with this comment. We reassessed that sentence and find it misleading. In our work we find that dry years are more recurrent since ~1980 and in the new version of the manuscript we highlight that this trend is previous to the mega-drought identified in Garreaud (2015) and Boisier et al (2016).

On the other hand, the positive trend of SAM can be related to any trend, even without a physical explanation. In your results, when you remove the linear trend the correlation fall to near-zero values, so what is the reason there is a relationship between SAM and streamflow at 38°S? SAM it is just a long-term trend? Why there is not relation at other timescale? Do you know what is controlling the SAM trend? That is a key answer to make. I think you should read the following paper:

<http://www.scielo.org.mx/pdf/atm/v25n1/v25n1a1.pdf>

RESPONSE: We do not completely agree with this assessment. First, we believe we provide a mechanistic explanation of the influence of SAM on precipitation in the region (Page 8 L.5 to Page 9 L.5). Nevertheless, we see that the recommended paper is very helpful for us in order to improve this explanation. Second, the relation between SAM and our data is not near zero; we performed correlations using a prewhitened version of the reconstruction and observations (removing the lag-1 autocorrelation) and they are negative and statistically significant (-0.287 with p-value=0.033 and -0.290 with p-value=0.029, respectively).

Minor comments

Page 2, lines 10-11. This values were taken from the figures? If so, how accurate is that?

RESPONSE: Yes, the data had been read from the figures. We reassessed that section and decided to utilize the information provided by the Atlas of Global and Regional Climate Projections of the IPCC (IPCC, 2013). We eliminated the sentence in Page 2-Lines 7-13 and replaced it with the following: “SCC is expected to undergo important climate changes. Analysis of the multi-model ensemble for the scenario RCP4.5 presented in the Atlas of Global and Regional Climate Projections (IPCC, 2013) indicates 10 to 30% reduction in spring and summer (October to March) precipitation by 2016-2035 and 2046-2065 relative to 1986-2005. The same projection forecasts 0.5 to 2°C warming for summer (December-February). Drier and warmer summers for may make SCC more vulnerable to water scarcity, given that this is the season of highest water demand in this region (Garreaud, 2015).

Page 2, lines 12-15. You have written 3 times "in this region" in few lines. I think it can be improved.

RESPONSE: Thanks for catching this up. We replaced the second “in this region” for “here” and the third for “SCC”.

Page 4, line 17. Where is mentioned Table 2?

RESPONSE: Thanks for finding this omission. We have mentioned the table besides Fig. 1

Table 2. What instrumental streamflow record have you used?

RESPONSE: This is from the composite time-series. We have clarified this in the caption.

Page 4, line 15. I am not expert on dendronology, so I can not questioning the methodologies employed to construct the index based on three tree-ring chronologies. About equation (1), however, there is something intriguing to me. I assume that water availability affects in the same way same species of trees. Why coefficients are opposite in sign for PAG (+2.69) and LYV (-1.97) at the same time (t-1)?

RESPONSE: We appreciate this comment as it allows us to provide a deeper explanation of our procedures. In order to fully understand the reasons behind this kind of multi-regression model it is important to consider that the climate signal of a given ring-width is product of climatic conditions of the same year but

from previous years as well, and they can be different in certain moments and as results of different locations of the chronologies. In our study region, climatic conditions influence ring-growth in two main ways: (1) Current and previous year(s) snow accumulation on mountain areas can delay the ring's growing season, increasing the likelihood of negative correlations (and LYV is located at high elevation). (2) In high elevation sites temperature can be a limiting factor for ring-growth, this is because increase in temperature in this region is related to less moisture and rainfall, possibly producing negative correlations. Thus, PAG and LYV correspond to different landscape, one at high elevations (LYV) where some variable (e.g. temperature or seasonal snow) can explain certain portion of the correlation with hydroclimatic observations, and another at low elevations (PAG) where the relationship between ring-growth and soil moisture is more direct because for instance precipitation is always liquid. What it is important to keep in mind is that they are significantly correlated with the observational record and the statistical model is skilled in representing the streamflow variability. Muñoz et al (2016) and Lara et al (2015) are other two examples where the statistical model presents coefficients of inverse signal in the same year.

Page 4, line 24. It is written "the return period or extreme low flows..." Did you mean "the return period of extreme low flows"?

RESPONSE: Yes, "of" is correct. Changed

Page 5, line 21. You defined summer as January-February for streamflow. So, what are the previous months for rainfall and what is the value for the not simultaneous correlation"?

RESPONSE: We find that your question gives us a good opportunity to further demonstrate the relevance of studying January-February streamflow. We ran correlations between observed/reconstructed streamflow with Temuco rainfall for each month of the previous year (Figure R1.E). These correlations are significant for December and February of the previous year (p-value <0.1 and <0.05, respectively) for the instrumental record. For the reconstruction, the correlation is significant for June and December (p-value <0.05).

Table 2 is analysed in page 5. I suggest to exchange numbers with table 3.

RESPONSE: We do not agree with this suggestion. Table 2 follows Table 1 in the sense the Table 1 presents the instrumental record and Table 2 provides the information for the analysis of that instrumental record. Then Table 3 appears because it is about the tree-ring chronologies. If we do the change suggested, we feel the manuscript loses readability and that our line of argument weakens.

Page 5, lines 26-27. Clearly, streamflow as precipitation exhibits a positive skewness in southern Chile, which is a normal behaviour taking into account that at most, there will be no rainfall (0 mm) as the lowest values. This kind of distributions are typical also for wind speed. So, I do not understand the point of this sentence.

RESPONSE: We believe this sentence is clear since it provides summary statistics corresponding to Table 2, which is about ranking streamflow extremes. We want to stress our analysis is about base-flow, which should rarely go to zero as rainfall and wind speed do.

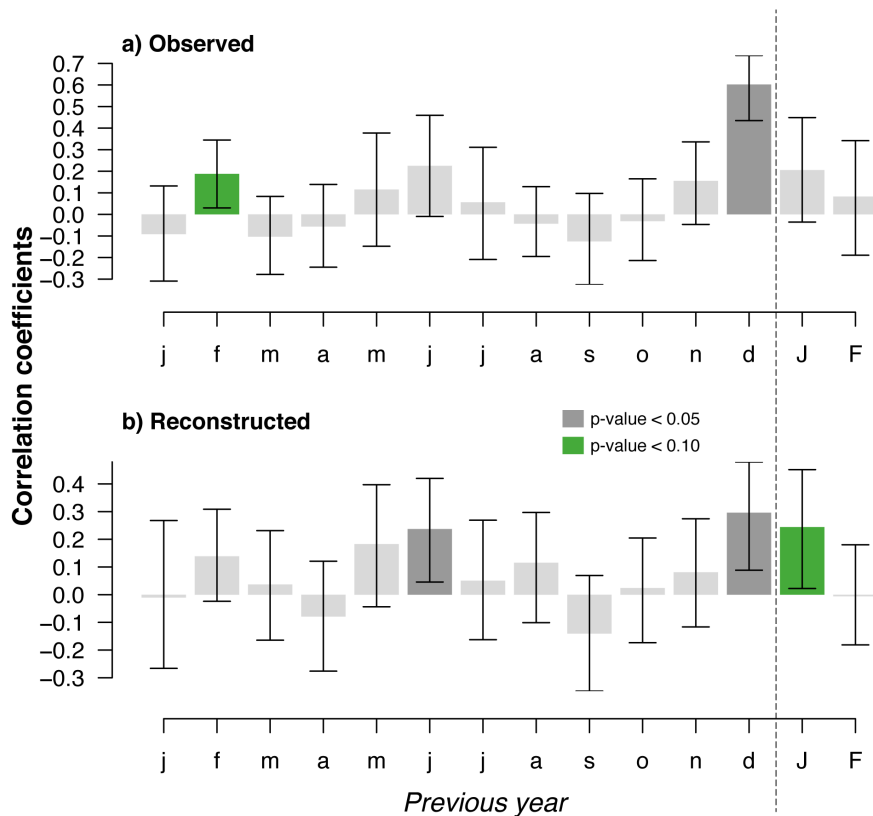


Figure R1.E: Correlation of observed/reconstructed streamflow with Temuco rainfall for each month of the previous year.

Page 6, line 8. Define VIF.

RESPONSE: We will include the following definition of VIF in the method’s section: The Variance Inflation Factor (VIF) evaluates the multicollinearity of the predictors; a VIF close to 1 means a low or no multicollinearity (Haan 2002) while a value above 10 is associated with multicollinearity problems between predictors (O’Brien 2007).

Fig. 4. It is very nice the percentiles at the bottom of the figure. Easy to interpret. I wonder what would be the percentile for the period 2010-2015? This information is available, why you do not have used?

RESPONSE: We updated that figure (Figure R1.B) in a previous response.

Page 8, lines 33-34. The summer of 1999 is part of La Niña, not El Niño. In fact, the winter of 1998 is one of the most dry winters in instrumental record.

RESPONSE: Thanks for catching this up. We are certain that winter 1998 was in fact part of a strong La Niña rather than el El Niño. We have modified the section “and the strong El Niño event of 1998” in the following: “ and a strong La Niña event in 1998-1999”.

Page 10, lines 24-25. The restriction of power supply occurred in 1996, at least in Santiago. It was different in Temuco?

RESPONSE: Our reference (Fischer and Galetovic, 2001) and the Decree 287 of 1999 (available at <https://www.leychile.cl/Navegar?idNorma=137602&idParte=>)

indicate that restrictions in energy supply were implanted across the whole Central Interconnected System in 1999, which includes Temuco.

New references

Decree 287 of 1999 (available at <https://www.leychile.cl/Navegar?idNorma=137602&idParte=>)

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